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THE PEACE CONFERENCE OF LAUSANNE, 1922–1923

JOSEPH C. GREW

(*Read November 12, 1953*)

As I wrote to Justice Roberts last spring, the invitation to address this Society is one of the highest honors that has ever come to me.

The purpose of the Society to review significant developments in all the fields of knowledge ties in with my own feeling that the accurate recording of history is one of the most important of the disciplines and this feeling has led me during my forty-odd years of public service to keep full personal contemporary records. These are comprised in some 168 bound volumes, which I have felt might some day add color and atmosphere to the perhaps drier official reports from our officers abroad. They are now in the Harvard Library. I have felt that an obligation rested on us, while in the arena, to make our individual contributions toward piecing out the more formal record.

This, together with the Society's further objective to develop a humane and philosophical spirit is my excuse, if excuse be needed, for my paper tonight on certain aspects of the founding of the present Turkish Republic on the wreckage of the former Ottoman Empire, for few events in history have surpassed in drama the enlightened transition in a remarkably short period of time from the old to the new and the alteration of "The Sick Man of Europe" to a progressive modern state conscious alike of its international obligations and of its opportunities to take its proper place in the family of civilized nations.

The Lausanne Peace Conference of 1922–1923, which led to the consolidation of the Turkish Republic, was one of the most dramatic international meetings that I have ever attended. How could it be otherwise when such well-known figures as Mussolini, Lord Curzon, Venizelos, Poincaré, General Ismet Pasha, later Ismet Inönü, President of the Turkish Republic, and other outstanding statesmen participated? After sitting for three months in the autumn of 1922 and the winter of 1923 the conference literally blew up and it was not until three months later that it reassembled, and finally, after another three months, produced a treaty of peace between Turkey and the Allies of the First World War.

We Americans, since we had never gone to war with Turkey, were present only as so-called observers, our duty being two-fold: First, to help in every proper way toward the attainment of peace and, second, to protect legitimate American interests in the Near East. During the first part of the conference our representatives were Richard W. Child, Ambassador to Italy; Admiral Mark Bristol, High Commissioner to Turkey; and the present speaker, who was then Minister to Switzerland. In the second phase of the conference he alone represented the United States.

The origins of the Lausanne Conference were historically of marked interest and significance in the development of Turkey's future world relations. The transition from the Ottoman Empire to the Turkish Republic was not an easy process. I well remember those early years of the Republic for I was closely associated with that process of transition. Those were the years of great reforms which profoundly affected Turkish life—the new law codes, new emphasis upon liberal education, the emancipation of women and many other progressive and constructive developments.

I remember the days when Angora was little more than an undeveloped provincial town, adjoining the ancient citadel, almost bereft of trees and with a mosquito-infested swamp on the outskirts. Malaria was rife. Washington was probably even more primitive when it was first chosen as our own nation's capital. In fact, thirty-three years went by before the foreign diplomats could be persuaded to move there from Philadelphia. In Angora our representatives there before my own arrival in 1927, Robert Imbrie and Howland Shaw, had to live in a railway car on a siding because modern habitation was unavailable or nonexistent. It was not uncommon in those days to see a half-dozen or more bodies hanging from the execution tripods in the main square of the town. Admiral Bristol, our High Commissioner, lived in Constantinople, now Istanbul. Politics seemed fairly simple in those days, too, for only one political party existed, and that was the Government party.

My own recollections take the form of a series of clichés. Far back, in 1905, I attended, as a visitor, the Selamlik, and saw the Sultan Abdul Hamid drive through the narrow streets of Constantinople to attend the Friday rites. Picturesqueness and beauty were there in full measure, the fascination of the mosques and bazaars in old Stambul, the perennial loveliness of the Bosporus. How keenly I remember the five summers later spent on that ineffably beautiful waterway. But in those days of the Ottoman Empire, it was not the beauty of a free society. The Turkish people were still in shackles.

Then came the First World War and eventually the Lausanne Conference. After the Allies had completed their peace treaty, General Ismet Pasha and I sat down at a small table and together negotiated a treaty between Turkey and the United States.

It was a good treaty, from both the American and the Turkish points of view. Even today, that treaty, although it is now as dead as a door-nail, is commemorated by a bronze plaque in the Beau Rivage Hotel at Ouchy, Lausanne. In some respects, indeed, it was more favorable to American interests even than the treaty negotiated by the Allies was to their interests because I simply outsat General Ismet. I brought it home to Washington, well pleased and expecting the accolade "Well done thou good and faithful servant." But, alas, alas, domestic politics intervened. There were still in our country elements which were dissatisfied that I had not been able to pull impossible rabbits from impossible hats. I was openly accused in the United States Senate of having sold my nation's birthright for a mess of pottage. The Americans of Armenian origin in the United States wanted to obtain all of Armenia as a sort of independent fatherland. The debate in the Senate was close but my treaty was finally defeated by six votes. Then, a few years later, I was sent as the first American Ambassador to the new Turkish Republic to negotiate a new treaty.

Ah, but times had changed then. By that time the Turks were riding high. My new treaty was duly negotiated in Angora. By that time the Armenians in our country had shot their bolt, and, while this second treaty was not one-half as favorable to American interests as the first one, it passed the Senate *viva voce* without even a tallied vote. Such are politics! But I was satisfied. The relations between Turkey and the United States were then on a firm treaty foundation and the ground was laid for future healthy development.

To go back to the origins of the Lausanne Conference, there occurred, during the war, the British invasion of Gallipoli. If my history is correct, the British Commanding General, after his troops had swarmed ashore on the beaches, allowed them some twenty-four hours to wash their clothes and rest, before proceeding to the attack. At that critical moment, the story has it, and I believe the story is accurate, the Turkish line across the Gallipoli peninsula had in its center a dangerous unfilled gap through which any invading army could have poured. This situation was reported to the German General, Liman von Sanders, in command of the defense of Constantinople, but his orders appear to have been delayed. And then it was that a comparatively young Turkish Colonel on his own initiative himself gave the order to fill the gap and moved his regiment into the unfilled area to complete the Turkish line. When the Allies arrived the next day, it was too late. The gap had been filled. Those twenty-four hours of rest and clothes-washing had been fatal to the attack. Perhaps I need hardly say that the name of the Turkish Colonel who had taken the responsibility and given the command to fill the line of defense was Mustapha Kemal, later President Kemal Atatürk, the George Washington of the Turkish Republic.

I will not take up your time by a detailed account of the developments of the next four years. Suffice it to say that in the Armistice of Mudros in 1918, after Turkey had capitulated, and in the grim Peace Treaty of Sèvres in 1920, Turkey, with two governments functioning, one in Constantinople and the other in Angora, was reduced to a helpless fraction of its former self. The Allies, being busy with other things, then apparently forgot all about Turkey and overlooked the fact that the Sultan had refused to ratify the Treaty of Sèvres and that the nationalists had simply repudiated it. Thus Turkey and the Allies were still at war.

In the meantime, the Greeks, incited largely by Lloyd George, had in 1919 invaded Anatolia and destroyed Smyrna. But the Turks, magnificant fighters that they were, led by Mustapha Kemal and General Ismet Pasha, drove the Greeks from Asia Minor in September, 1922, and the Armistice of Mudanya was signed on October 11, 1922, by which Turkey retained all of Anatolia and Eastern Thrace. It was in that situation and atmosphere

that the Lausanne Conference was called in the autumn of 1922. By then the Turks were full of confidence, as they had good reason to be. Some of the Allies failed to recognize the great change that had come about in Turkey since the Armistice of Mudros and the Treaty of Sèvres. They pounded the conference table and demanded one concession after another. General Ismet Pasha sat quietly and simply said "no" to almost everything. He was in the driver's seat and knew it. The Allies had no stomach to fight so soon again.

Today a strong Turkey stands out as a force for stability in the Eastern Mediterranean area. This is a Turkey determined to maintain its national integrity, but mindful of its responsibilities to cooperate with other freedom-loving nations to create the kind of world in which it can survive as a free nation and proceed with its national development.

The announcement in 1945 by President Inönü that new political parties to supersede the single Government party would be permitted in Turkey was a tangible indication of the consolidation of the Republic. In recent years, the multi-party system has come to be recognized as a permanent feature of Turkish political life. While united on foreign policy, the various parties are expressing themselves openly in the Grand National Assembly, in the press and in public meetings, on the differences which separate them in internal affairs. They, as well as the Turkish people, have been gaining valuable experience in democratic processes, learning the necessity of getting along with each other, even though holding opposing views, in such a way as to promote rather than endanger the national security. The world will watch with great interest further evidence of the maturity of the Turkish people.

THE LAUSANNE CONFERENCE

At the very beginning interest, of course, centered on Mussolini, the new hero, untried and unknown. He himself evidently had a view to the dramatic and was anxious to impress the people at home with his independence among foreign statesmen. Instead of coming straight to Lausanne, he stopped at Territet and made Poincaré and Lord Curzon come to see him there. In the course of the evening he agreed to meet them in Lord Curzon's room at Lausanne the next morning punctually at ten o'clock. At 10:20, after Poincaré and Curzon had fumed up and down the room waiting for him, they sent a secretary to see what was the matter. The Secretary, who told me the story, said he found Mussolini quietly reading in his room and, when he told him that he was twenty minutes late for his appointment and that the two other statesmen were waiting for him, Mussolini said: "Oh! they're there, are they? Very well, I'll come." On another occasion, he sent a representative to Lord Curzon to announce that he would dine with him that evening. Curzon sent back word that he would be very glad to have Mussolini dine with him at 8:30. The messenger remarked that he was sorry that he had instructions to say that Mussolini would dine with Lord Curzon at 7:30. Curzon threw up his arms in despair and agreed to split the difference and call it 8:00. But quite apart from these little by-plays, I was deeply impressed by Mussolini's quiet reserved force and the great power of his personality.

A story is told that when Claire Sheridan, the authoress and sculptress, turned up at Lausanne, Mussolini jokingly invited her to come to see him in Rome and that she promptly accepted the suggestion, traveled by the same train and took rooms in the hotel where he was staying. Mussolini still regarded the matter as a joke until the day after their arrival several men appeared at his room with many pails of plaster, boards, nails, etc., and when he asked what in thunder they were for, he was informed that Mrs. Sheridan was about to make a bust of him. Mussolini forcibly put the men to flight and is said to have written Mrs. Sheridan a letter in which he refused positively to be sculped adding that busts were made only of dead men and that he himself was still very much alive.

There was the late Mr. Stamboliski, the Prime Minister of Bulgaria, hairy, enormous, with fierce upturned moustaches, looking far more like a brigand than a statesman, sitting stolidly at the conference table with his secretary, Mlle Stancioff, of the Bulgarian diplomatic service, just behind him interpreting his words and, I imagine, also his thoughts, because for a half dozen words from her chief Miss Stancioff would talk perfect French or perfect English for ten or fifteen minutes. She was witty and had many passages of arms with the other delegates. One day, when pleading for Bulgaria's outlet to the sea, she said to Venizelos "How can you, with your hundreds of harbors, object to the legitimate desires of poor little Bulgaria, which has but three miserable ports on the Black Sea?" Venizelos, as quick as a flash, replied "God put Greece in the middle of the sea; it is not my fault!"

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There was Marquis Garroni, the first Italian delegate, aged, genial, soothing as a lullaby at the conference table, but with never a word that was original or constructive. He simply tagged along.

There was Barrère, the old-school French diplomat, who had been Ambassador to Italy for some twenty-five years. It is said that in his youth he was a rabid communist and that when he presented his letters of credence to King Humbert, the latter asked him how it was that such a change had taken place in his political ideals. Barrère replied: "Sire, a man who is not radical in his youth has no heart, but a man who is not conservative in his age has no head."

There was Tchitcherine, the Russian delegate, who talked in a high, squeaky voice which reminded me of nothing so much as a slate pencil. During the conference I was invited by my Norwegian colleague to meet him at dinner, in an entirely unofficial way, as he had particularly asked to meet the American representative. My inclination at first was to refuse, but my colleagues urged me to accept on the ground that something useful might be learned. And so I went. There were five of us at table, the three Scandinavian Ministers, Tchitcherine, and myself. We sat there for four hours and the talk was intensely interesting. We discussed fascism at length and Tchitcherine's summing up of Mussolini was "He has a passion, not a program." Once, in speaking of the conference, he turned to me and said: "Every rebuke Lord Curzon addresses to me across the conference table strengthens me just so much with the Soviet Government and strengthens the Soviet Government proportionately." In reply to my inquiry as to whether developments were taking place in Russia tending towards a more moderate and less radical regime, he said: "Yes, but don't tell the people so. The people must not be allowed to suspect what is going on in this direction. If they were openly aware of it, further developments would become impossible."

But the outstanding personalities of the first phase of the conference were, in my opinion, Lord Curzon, Venizelos, Montagna, the second Italian delegate, and Ismet Pasha. Curzon impressed me greatly. He was said to be pompous, conceited, and a martinet to his subordinates, who still refer to the old college legend that "George Nathaniel Curzon is a very superior person." But in his personal relations at the conference we saw nothing whatever of this kind. He was always genial, always courteous, always entertaining. Never have I enjoyed anything more than the small dinners of three or four which he appeared to love, and where he would sit hour after hour telling stories, anecdotes, experiences, in a delightful vein. But at the conference table his attitude and tactics were, in my opinion, ill-judged and productive of little success. He habitually treated Ismet like an office boy, browbeat him, and covered him with ridicule. Ismet bitterly resented this, and it did not make for a spirit of conciliation on the part of the Turks. At the very beginning Lord Curzon adopted steam-roller methods and refused to allow the Turks to have any say in the organization of the conference. One decision after another was adopted over Ismet's protests and, when at the next meeting Ismet was faced with a "fait accompli" and ventured to protest, Lord Curzon simply remarked that objections were now too late and went on to the business of the conference. Whenever Ismet mentioned Turkey's national aspirations or her desire for independence and sovereignty, Curzon always waxed facetious. He once said "Ismet, you remind me of nothing so much as a music box, for you play the same old tune over and over and over again—sovereignty, sovereignty, sovereignty." On another occasion he said "Ismet, you have a perfect mania for believing we are depriving you of your independence and you have thought about it so much that you really believe it is true. You remind me of King William who used to tell his friends that he had led the charge of the guards at Waterloo and he told it so often that he finally came to believe it himself, although, as is well known, he was fully one hundred miles away when the battle took place." Curzon was equally facetious with the Russians, and once, when Tchitcherine suggested that the discussions regarding the Straits were making no progress and that the best thing to do was to get down to work and draft a treaty, Curzon immediately replied "Monsieur Tchitcherine, that is a most excellent suggestion. If you will shut yourself up in a room all alone and draft us a treaty, I can assure you that we shall give it most careful consideration, and I have no doubt that it will prove to be a document of most unusual interest." But apart from his sarcasm and irony with the Turks and the Russians, Curzon was an admirable Chairman. It was a delight to listen to his clear, incisive remarks, his beautifully chosen words and phrases, and his well-rounded sentences. At the conference table he was never for a moment at a loss. Once, when asking that

Turkey deed to England the ground where the British dead lay buried at Gallipoli, and Ismet remarked that there was no precedent for such action in history, Curzon immediately replied that the British Government had deeded to France in perpetuity the house and ground where the Great Napoleon died on the Island of St. Helena and that the French flag perpetually flew from the staff of the building.

As to Venizelos, he was the fire-brand throughout and, although often illogical and frequently inaccurate in his arguments and remarks, everyone paid attention when he spoke. He would begin gently and in a soft ingratiating voice, but little by little would work himself into a fury, bellow at the top of his lungs and wildly wave his arms in the air, which, as I was seated next to him, was not always conducive to my personal comfort. On one occasion during one of these transports of fury the Chairman said "I beg of you Mr. Venizelos to be tranquil." Whereup Mr. Venizelos beat the table with his fists in redoubled violence and shouted at the top of his lungs "I am tranquil, I am tranquil." Riza Nour Bey, the Turk, then turned the solo into a duet and proceeded to shout at Venizelos across the table. Montagna, the Chairman, pounded on the table with both fists trying to restore order, but finding it impossible he declared the meeting adjourned. This however had no effect on Venizelos who had worked himself into a frenzy. Indeed his passionate duels across the table with Riza Nour Bey were the delight of the conference.

One day a dramatic incident was provoked when Tchitcherine said he hoped Rumania would be careful and not allow herself to get into the position of Greece which had suffered so severely at the hands of the Turks. Venizelos immediately challenged him saying that it was not customary at such international conferences to make cryptic remarks of this nature and that he demanded an explanation. Tchitcherine shuffled his papers, consulted his experts and made a rather lame reply that he was referring only in general terms to the international situation, at which Venizelos shrugged his shoulders and said that he would not press the point as he did not wish to delay the procedure. Whether we sympathized or not, we all listened when Venizelos spoke. He was a man who would command attention in any situation and his free, self-confident, clear delivery was a blessed relief after the halting indistinct speeches of some of the other delegates.

I mentioned Signor Montagna, the second Italian delegate, as one of the outstanding figures of the conference because he understood the mentality of the Turk and the proper method of dealing with him better than any other delegate; and, not only did he bring the work of his sub-committees to a satisfactory conclusion while other committees were still deep in acrid debate, but at the critical moments of the conference it was always he who, in a quiet and unobtrusive manner, managed to smooth matters over by his sensible and conciliatory tactics. At the end of the conference, after Montagna had, in my opinion contributed more than any other delegate toward reaching a final agreement, old Marquis Garroni insisted on coming up from Rome to sign for Italy, although he was the only one of the original first delegates to do this. And thus Montagna was deprived of the prestige, if it could be regarded as such, which he had earned by indefatigable work. In the darkest hours of the conference Montagna was always optimistic, and I am convinced that on more than one occasion he personally prevented a rupture.

As for Ismet Pasha, my opinion of him underwent several changes during the course of the conference, but my final judgment is that, while lacking any of the adaptability or finesse of his opponents, he played a straight and honest game throughout. He was often accused of giving his word and then withdrawing it, but it must be remembered that he was continually subject to instructions from Angora, which I have every reason to believe were frequently more intransigeant than he himself wished to be, and, at least in our own negotiations, when, from time to time, he tentatively accepted a certain formula or provision for our treaty and later withdrew his agreement, he always made it clear in advance that these provisional agreements were subject to instructions from home. In the course of our negotiations, when we were debating some complicated point, he would frequently say to me "Mr. Grew, you are a diplomat while I am nothing but a soldier and I am not versed in the finesse of diplomacy"; to which I always replied "Every day of my twenty years' experience in diplomacy has shown me the advantage of expressing one's mind openly, candidly, and honestly, and you will never find any other kind of diplomacy on my part." After all, in Ismet's position, it was the bluff qualities of the soldier and not the subtlety of the old-school diplomat which he needed and

which he exercised throughout. He was in a besieged stronghold and all he had to do was to hold out against all assaults and he did it with conspicuous success. In actual debate he was hopelessly outclassed, partly through the handicap of his deafness, which necessitated every word that was said at the conference being written down for him by the secretary at his side, and, also, I believe, because he was neither quick-witted nor keen of comprehension; nor did he appear to have a thorough grasp of the details of the subjects under debate, and many a time at the conference table he missed an admirable opportunity to score off Lord Curzon and others when being attacked and browbeaten. I remember once when Lord Curzon was attacking Ismet's objection to the continuance of the foreign stationnaire ships in Constantinople on the ground that they were a relic of the capitulations, Lord Curzon said "But our only reason for keeping the stationnaires is for the purpose of obtaining transportation to and fro like cabs or taxi automobiles." What a hit Ismet would have made if he had replied "Then, Lord Curzon, I assume that you will have no objection to our maintaining a Turkish stationnaire in London, which is also a port and where transportation is equally as necessary as in Constantinople."

The element of intrigue and international rivalry which went on at Lausanne, of which I had occasion to observe many instances and illustrations, opened my eyes to the sorry state of old world diplomacy and caused me to realize that even the plots of E. Phillips Oppenheim are not all of them so far-fetched. During the course of the first phase of the conference a room of one of our secretaries was entered, his locked trunk broken open and an envelope bearing the title and seals of the Department of State was stolen and never found. Fortunately, it contained nothing more comprehensive than the *laissers-passers* and personal accounts of the secretary in question. But, from that moment, we arranged to have one of our staff in the Chancery day and night, without exception.

But I must not dally with these various sidelights and must tell you something of the working of the conference. On January thirty-first Lord Curzon decided that drastic measures must be taken to conclude negotiations and he not only presented the Turks with a complete draft treaty based in part on the agreements already reached, but containing many other provisions which had not been fully discussed, and he also drew up a time table mapping out each day's procedure and ending with his definite determination to leave Lausanne on February second, which was later extended to February fourth. To make a long story short, I can perhaps not do better than read to you the entries in my diary for that last day of the first phase of the conference.

Sunday, February 4, 1923.

A fateful day. None of us had any idea what it would bring forth. We simply knew that the Allied draft treaty was to be on the conference table at 4 P.M. for the Turks to sign or leave, and that Curzon had sworn to depart at 9 P.M. whatever the result. Duca, the Rumanian Minister for Foreign Affairs, said to Mrs. Grew just before lunch, in a particularly choice metaphor: "The abscess is going to break today but we don't know whether the result will be good or bad."

At 1:30 while we were all at lunch the first important development occurred. I saw Arlotta, the Italian secretary, hurry into the dining room and hand a document to Garroni who immediately arose and left the room. It proved to be a draft treaty drawn up by the Turkish delegation and handed to the Allies in reply to their draft, containing only those clauses which had been discussed and agreed to by both sides in committee and omitting all others. Ismet accompanied it by a note in which he said that he felt the clauses already agreed to constituted a sufficient basis for peace, and that other subjects still under controversy could be settled later. It was a clever move.

The delegates of the Inviting Powers immediately went into conference in Lord Curzon's room. At 3:30 they summoned Ismet who came down from his hotel with a large group of his experts. At 7 Bentinck, the British secretary, called me on the telephone and said that the Turks were probably going to sign in a few moments and suggested that I get our delegates together to be ready to go to Lord Curzon's room to witness the ceremony. I collected Ambassador Child and Admiral Bristol and we went into the hall at the foot of the staircase in the old part of the hotel leading from Curzon's floor. The hall was packed with members of delegations and newspaper correspondents waiting for the final dénouement. The air was full of electricity; hardly anyone talked; we simply listened and waited. The hall upstairs was littered with the packed trunks of the British delegation; in fact when Ismet was calling on Curzon yesterday, the British made a point of bustling about and removing various trunks under Ismet's nose so that he could not fail to be impressed with the definite plans for departure. Thus we waited, expecting any moment to be summoned to watch the

signing of the treaty. Suddenly, at 8 o'clock, the sound of a door opening above; everyone got up and moved toward the staircase. In a moment Ismet appeared, descending the stairs followed by his delegation; he took off his bowler hat, bowed right and left to the crowd in the hall, smiling broadly and left the hotel. Certainly I shall never forget that scene. The Conference was broken; there would be no signing. After Bentinck's message an hour before we had hardly doubted that a settlement would be made. Child, Bristol and I almost immediately went to Lord Curzon's room. Everyone had left. In a moment Curzon appeared; he burst into the room like an angry bull, glared at us and began to pace up and down waving his fist in the air. He was perspiring and looked all in. He shouted, "We have been sitting here for four mortal hours and Ismet has replied to everything we have said by the same old banalities—independence and sovereignty. We have all done our best. Even Bompard (the French delegate) beat his fist on the table and told Ismet that he was simply stirring up war. Bompard made the strongest speech I ever heard him make." We asked Curzon what Ismet had broken on. Curzon said it was the judicial clauses. (This was only partially correct as it was the economic clauses too.) He said that at the last moment Ismet had withdrawn to another room with his experts and that they had fully expected him to come back and sign. Instead he returned, refused to sign, bowed and left the room. It was all over. Curzon was in a fearful state. We asked him if we could do any good by seeing Ismet, for as impartial observers it was within our province while safe-guarding American interests to contribute in every proper way to the cause of peace. He said that Bompard and Montagna had gone up to see him but that it could do no good. We said we would try and again asked him if the judicial clauses were the real stumbling block. He answered in the affirmative.

Child, Bristol and I then hurried downstairs. I had the Hudson at the door in a minute and we drove quickly to the Palace Hotel. Bompard and Montagna were with Ismet but we were shown into Hussein's room. The concierge told us that Lord Curzon's train, the Orient Express, was an hour late and would not leave before ten o'clock. This proved to be wrong. Curzon himself had had the train held back for half an hour in order to hear the result of Bompard's and Montagna's talk with Ismet, but as soon as they reported no results the train was released and actually left at 9:25 or thereabouts.

A moment after we were seated Ismet came in, Bompard and Montagna having just left. We were with him approximately an hour. We went over much ground with regard to the judicial clauses, but Ismet told us at once that the economic clauses were no less the cause of the break as they would place Turkey in "financial and industrial slavery." We asked him if he would make further concessions on the judicial clauses provided we should obtain concessions from the Allies in other respects. He asked what we suggested. We said that first of all the foreign judicial advisers should be admitted not only to the courts of Constantinople and Smyrna but to those of Samsun and Adana as well. The argument continued for about half and hour; Ismet kept rubbing his forehead as if almost dazed. He said in Turkish "My heart is squeezing me." Finally he got up and went into the adjoining room, stayed there for three minutes, then returned. It was evident that he had gone to consult his experts, but he continued the discussion without a sign that he had seen them. Then quite suddenly he said, "All right, I will concede." We said "Samsun and Adana?" He replied, "No, only Samsun." We said: "That is not enough to justify our retaining Lord Curzon." Another twenty minutes of discussion. Again Ismet left the room—for we were pushing him hard—returned and seated himself again. He looked awfully tired. Then after a decent interval, so as not to give us the impression that he had talked with his experts, he said: "All right, I will give you both Samsun and Adana, but you must give me your word of honor that you will not reveal these concessions to the Allies before you have secured theirs." We agreed, arose, shook hands and said we hoped and believed that this would be a fair basis for renewed negotiations and hurried to the station in the car. We were elated for these concessions that we had obtained were by no means unimportant, Ismet having obstinately stood out on them with the Allies. We felt sure at that moment that Curzon would remain.

At the station I saw Bompard getting into his car and wondered why he was leaving just as Curzon was about to start. Other people appeared to be entering their cars also and I saw some of the Italians coming out of the station. I went up to McClure who was near our car as we got out and asked him how soon Lord Curzon's train was starting. "He's just gone," he said; "The train has just pulled out. They've all gone—the whole delegation." And that's that.

It is idle to speculate upon whether the Conference of Lausanne could have been saved by our arriving five minutes earlier at the station. It is not impossible; in fact I hardly see how Curzon would have dared to leave after what we had to tell him, for it showed that the Turks had not got to the end of their rope in the matter of concessions and that with a sufficient amount of patience still other concessions might be gained. Patience was the necessary element which Curzon lacked and now I know that without that quality it is useless to try to deal with the Turk.

The second phase of the Conference, which began on April 23 and ended on July 24, was in many respects far more interesting than the first phase, but I do not wish to weary you with too long an account and shall therefore limit myself to speaking of only two or three of the outstanding features. The first difficult problem we had on our hands was, strange as it may seem, the necessity of establishing friendly relations with our British colleagues. Apparently Lord Curzon had gained the impression, during the first phase of the Conference, that our delegation had been distinctly pro-Turk and had aided and abetted the Turkish delegation in their Machiavellian manoeuvres. It is true that one or two members of our delegation, who had lived long in Constantinople, were somewhat pro-Turkish in their attitude and were perhaps seen too frequently in the company of members of the Turkish delegation, which may have given rise to these unwarranted suspicions. But the charge that our delegation itself was pro-Turk, or took any step, or said any word contrary to the interests of the Western Powers was wholly without foundation. Throughout the entire Conference we lived up to the purpose of our mission, which was (1) to safeguard American interests and (2) to contribute in every proper way towards the conclusion of peace.

Be that as it may, we learned through various channels that the British Government was not anxious that we should be present at the second phase of the Conference. The French and Italian Governments, however, took a different view of the matter and, when the notices for the second meeting were sent out by the Secretariat General, we duly received our notice with the rest, and, to my considerable apprehension, I was appointed to represent the United States alone. At the very beginning I made a point of seeing each of my Allied colleagues and telling them exactly where we stood and what we proposed to do at the Conference, and that I should take no step without giving them full information. Thereafter, whenever I saw Ismet, which was, of course, promptly known to all, I took immediate occasion to see my Allied colleagues and tell them exactly what we talked about. To make a long story short, this policy had its desired effect, because, after having strenuously opposed our request to be admitted to the deliberations of the various sub-committees and groups of experts, Sir Horace Rumbold, the British representative, finally withdrew his objection sometime after the Conference had started. Instead, however, of coming to me and telling me frankly of his change of attitude, as I should certainly have done in a similar case, he had to save his face at all costs and merely sent his legal expert to break the news to me. About the same time he called upon me with great ceremony and, taking a document out of his pocket, he said that he had been instructed by his Government to read to me the following message:

> His Majesty's Government values the friendship and cooperation of the American Delegation and will be glad to learn its views and to inform it of our views during the course of the Conference.

This was the only call that Rumbold made upon me during the entire three months that we were colleagues at Lausanne, although we had been young secretaries together in Cairo some twenty years ago and were again colleagues in Berlin before the war when we together sealed up the archives of the British Embassy. The French and Italian delegates, on the contrary, came to see me as often as I went to see them, and we were in the habit of dropping in on each other at any time of day or night to compare notes and discuss developments. In spite of the message from the British Government, Sir Horace Rumbold never once during the Conference consulted me or gave me his own opinion or information on his own initiative while we were in Lausanne. As a result he missed much that might have been of value to him, while the information he could have given me I was able to obtain equally well from other sources. These things count in the long run. It makes one inclined to agree with a remark made to me one day by one of my other colleagues at Lausanne that "the French are clever in little things but stupid in great ones, while the British are clever in great ones and immensely stupid in the small things."

The most serious development in the second phase of the Conference of Lausanne was unquestionably the Turco-Greek dispute over reparations. There is no doubt whatever that at one moment the Conference hung on the brink of rupture and Europe on the brink of war, for if the Greek army had invaded Eastern Thrace, as it was certainly on the point of doing, there would without question have been a general conflagration in the Balkans, and there is no telling how far that conflagration might have extended. The danger became evident about May 10 on which date I learned that the Greek Government was consider-

ing an ultimatum to Turkey on the following grounds: (1) Maltreatment of Greek prisoners; (2) the opening by the Turks of safe deposit vaults in the Greek banks in Constantinople, and (3) the alleged expulsion of Greeks from Asia Minor. Venizelos, however, considered these points weak and recommended to his Government that the ultimatum, if issued, should be based on the Turkish claims for reparations from Greece. Shortly afterwards Venizelos called on Ismet and talked to him in a most threatening and bellicose manner. Ismet said, "Do you mean you are threatening me with war?" Venizelos calmed down somewhat and offered to agree to the principle of paying reparations, but with the understanding that no cash should be paid, as Greece was incapable of doing it. Ismet proposed either arbitration or the payment of a lump sum, both of which suggestions Venizelos refused. At this stage Ismet was visibly anxious and asked permission of the Allies to send Turkish troops into Eastern Thrace to meet the threat of war, which was, of course, refused. About this time Alexandris, the Greek Minister of Foreign Affairs, arrived in Lausanne and took an even more bellicose attitude than Venizelos had done. He said that Greece was absolutely determined to go to war rather than pay, and he remarked to Montagna that, as the Allies had supported the Greek offensive before, they could not now desert her. Montagna corrected him by saying "certain Allies" and added that "victory was often more costly than defeat." Alexandris said that the Greek army was the stronghold of the Allied position at the conference and was now at the zenith of its efficiency, but that if time went by without its attacking this efficiency would decrease. Montagna indignantly refuted the suggestion that the Greek army had anything whatever to do with supporting the Allied position. Alexandris then went so far as to say that the Greek army had been deprived of victory and that its officers now demanded satisfaction by invading Eastern Thrace. There is no doubt that this whole question was one of internal Greek politics. The military party was literally champing at the bit while Venizelos' party also needed some concrete asset to retain power and they were clearly pushing him toward war.

During these days the Allies had continual meetings to determine what should be done. I called on Rumbold at this time to ascertain his opinion, and he said he considered the situation dangerous but not necessarily critical. With the French and Italian delegates I worked in constant cooperation.

Pellé, the French representative, about this time proposed to Ismet that the Turks should accept the town of Karagach from the Greeks in lieu of reparations.

On May 25 I called on each of the Allies and said I could not see war intervene without exerting the impartial influence of the United States as a totally disinterested power, and that I proposed on my own initiative to extend my unofficial good offices for mediation. Each of the Allies agreed heartily to this proposal.

In spite of continual insistence on the part of Venizelos and Alexandris that a final meeting should be held, and, in spite of repeated threats on their part to leave Lausanne and to invade Eastern Thrace unless satisfaction were immediately given, every effort was made to postpone the meeting until Ismet should have had time to receive an answer from Angora regarding Karagach. On the twenty-fifth of May it was evident that the meeting could be no longer postponed. I spent the greater part of the night before the meeting with Venizelos and Ismet separately, using every possible argument to induce a peaceful solution. Venizelos told me that he would accept the Karagach proposal and I so informed Ismet.

The meeting was held at five o'clock on the following day with one delegate from each country present. It took place in a small room at the Chateau and was intensely dramatic. We all sat close together at a small table; Pellé, the President, and Ismet sitting opposite one another. Diamandy placed himself between Ismet and Venizelos with a view to separating the principles in the controversy. The meeting began with the utmost solemnity. Each delegate in turn made a speech emphasizing the seriousness of the situation and calling for conciliation and moderation on both sides. Rumbold used calm logic, Montagna made an impassioned plea, while Pellé summed up the situation in a masterful address followed by the Japanese, the Serbian, the Rumanian, and myself.

Ismet then began to talk and it was evident at once that he was trying to dodge the issue. Diamandy slipped a note across the table to Pellé begging him to pin Ismet down to facts. Pellé then asked Ismet whether he had telegraphed the Karagach proposal to Angora. Ismet replied in the affirmative. Pellé said, "Have you received a reply?" Ismet answered, "Yes." "Do you accept?" Ismet again tried to ramble off on

tangents, but Pellé persisted and finally, after every effort to avoid the issue, he gave a weary assent.

All this had lasted two hours during which we were all under the greatest possible strain and when it finally appeared that a solution was in sight, the psychology of human nature took its natural course and the air of solemnity changed immediately into one bordering on actual hilarity. Diamandy, the Rumanian, got up and insisted upon Venizelos moving next to Ismet. The details of the settlement were then discussed in the most amicable way; Venizelos and Ismet calling each other "mon cher ami" had their hands on each other's arms, laughed like school boys, and appeared to be on the point of actually embracing. The Serbian danced about the room recording his protest against the cession of Karagach, but assuring the meeting that he would not think of letting this stand in the way of peace and that he merely wished to go on record. Rumbold, the Englishman, showed his wild enthusiasm by a contraction of the facial muscles which amounted almost to a smile and the Jap beamed benevolently through his glasses as though he had just eaten a most excellent dinner and felt at peace with the world. Just before the end of the meeting Venizelos, on behalf of Ismet and himself, thanked all those who had helped in reaching this happy solution, including the American "Observer" and we all got up and shook hands all round. And that's about the whole story.

DR. JOHN FOTHERGILL, PEACEMAKER

BETSY COPPING CORNER AND DOROTHEA WALEY SINGER[1]

LET us draw aside the curtain of the past and with the powers conferred by historical imagination, which include the privilege of being invisible spectators for a brief period, let us look at an eighteenth-century scene. The time is late afternoon in December, 1774. The place to which we are transported is No. 5, Harpur Street, near Red Lion Square, Bloomsbury, the residence of Dr. John Fothergill, the eminent London physician and Quaker philanthropist whose interest embraces all that happens in the North American colonies.[2] In the doctor's candle-lit study our attention is distracted, as we enter, by his books in mellow leather bindings which line the walls of the room and by his cabinets of shells and curios which betray their owner's passion for natural history. Our attention soon concentrates upon a group of three soberly-dressed gentlemen, two of them evidently Quakers from their garb, who are seated at a table. The third member of the group, who seems to be the eldest, puts on his spectacles, takes up a paper from the table and begins to read in a firm voice various propositions:

1. The tea to be paid for.
2. The Tea-duty Act to be repealed, and all the duties that have been received upon it to be repaid into the treasuries of the several provinces from which they have been collected.
3. The Acts of Navigation to be re-enacted in the Colonies.
4. A naval officer, appointed by the Crown, to see that these Acts are observed.
5. All the Acts restraining manufacturers in the Colonies to be repealed.

The voice goes gravely on. Gravity deepens on the faces of the listeners as their ears pick up from the reading certain phrases: no requisition in time of peace; no troops to enter and quarter in any Colony but with consent of its legislature; Massachusetts and Quebec Acts to be repealed; free government for Canada; Britain to give up its monopoly of American trade; all powers of legislation to be disclaimed by Parliament. Seventeen separate points are enunciated by the reader of this document which covers a far wider field than the question of taxation without representation.

We have been listening, of course, to an American presenting the colonial side of an argument over a situation which will lead ultimately to the War of Independence. As we look closely at the reader of the document we shall discover that he is Benjamin Franklin of Philadelphia—or we should more properly call him Dr. Franklin since it is twelve years and more ago that he received his D.C.L. from Oxford at a convocation on April 30, 1762. He is a man who has both supporters and detractors in England where he has long resided as accredited agent for several of the Colonies. He has prepared the paper just read at the request of his host, Dr. Fothergill, and has called it by a ponderous self-explanatory title: *Hints for Conversation Upon The Subject of Terms that Might Probably Produce a Durable Union Between Great Britain and Her Colonies.*[3]

The three members of this early Anglo-American conference form an interesting contrast. Dr. Fothergill is sixty-two; Dr. Franklin sixty-eight; David Barclay, the third member of the group, only forty-six. Barclay like Fothergill is a Quaker. He belongs to the world of business where he has been successful as merchant and banker, with special activity in the North American trade. His interest and effort will be directed toward a practical solution of the current misunderstandings, since he is always conscious of the importance of commercial interplay between mother-country and Colonies. Dr. Fothergill, outwardly the calm professional man, conceals inner tensions, for he burns with the ardors of religious and political idealism. Benjamin Franklin, known as a shrewd experienced statesman, must feel the

[1] This paper was delivered by Mrs. Corner in London May 6, 1953, at a meeting of the Historical Section of the Royal Society of Medicine.

[2] Dr. Fothergill was elected foreign member of the American Philosophical Society in 1770, during the second year of Benjamin Franklin's presidency.

[3] Van Doren, Carl, *Benjamin Franklin's autobiographical writings*, 354, 355, New York, Viking, 1948.

pangs of divided allegiance since he loves England, its amenities and traditions, but America is his native land. Before the discussions come to an end it is he who will state the view that animates all three men:

> It is supposed to be the wish on both sides not merely to put a stop to the mischief threatening the general welfare, but to cement a cordial union and remove not only every real grievance but every cause of jealously and suspicion.

All very well in theory; in fact each side will stand resolutely by its own principles, stubbornly refusing any compromise acceptable to the other. In the face of this predicament our peacemakers in Dr. Fothergill's study might as well have tried to sweep back the tide. We must leave them, sitting at their table in quiet desperation and perplexity. As the weeks go by that winter, they will try to re-shape these *Hints* into terms suitable to form the basis for the establishment of peace. They will meet again and again, but by mid-February they must sadly admit that the negotiations they have undertaken are meeting with failure.

Of what took place at these meetings we have only one contemporary account available in print. Late in March, 1775, Dr. Franklin sailed with a heavy heart from Portsmouth for Philadelphia. To while away the tedium of a voyage lasting almost seven weeks, he wrote an account of his last months in England in the form of a letter to his son, which appears in collected editions of his writings under title of *An Account of Negotiations in London for Effecting a Reconciliation Between Great Britain and the American Colonies.*[4] It is a complex story of many threads woven into a tapestry of many scenes. We must discard from present consideration the accounts of his visit to Lord Chatham at Hayes; of the return call of Lord Chatham to Franklin's lodgings in Craven Street; of the ride with Lord Sandwich to Parliament to hear Chatham's speech with its recommendation of withdrawal of royal troops from Boston. We cannot linger, as we might like, over the pros and cons of Lord North's "conciliating" motion in the House of Commons. We must select for our present study only what is immediately relevant to the attempt made by Franklin, Fothergill, and Barclay to secure terms basic to reconciliation between Great Britain and America.

How the affair actually began, who first proposed this series of Anglo-American conferences is shrouded in mystery. Franklin's account starts with an invitation he received to play chess with Mistress Mary Howe at her home in Grafton Street. Although Franklin had no previous acquaintance with this lady he was well aware of the prominence of her family. Three Howe brothers had distinguished themselves in service to the realm. George, the eldest, had met with a hero's death in America during the Seven Years War; William, the youngest, with better luck had risen to the rank of Major-General; Richard, now the fourth Viscount of his line, at this time a man of forty-eight at the height of his capabilities, had been created Rear-Admiral in 1770. Franklin found their sister accomplished, socially agreeable, and thoughtful, a combination he could never resist in woman. Describing her, he wrote with some surprise: "This lady (which is a little unusually in ladies) has a good deal of mathematical knowledge." Conversation followed chess; problems of politics with life-size figures succeeded problems of pawns and knights and kings on the chess-board. The threat of probable trouble with America, which might lead to the outbreak of "civil war," as it was generally termed, was on everybody's tongue. The very day before the chess games began, in fact, Franklin had been engaged in conference on how to avoid this danger with David Barclay, the sponsor of a merchants' petition, signed by many substantial traders, who were protesting against any interruption of their lucrative colonial commerce. To Franklin, Barclay had then spoken "of the great merit that person would have who could contrive some means of preventing so terrible a calamity and bring about a reconciliation." He had added that Franklin "from his knowledge of both countries, his character and influence in one of them and his abilities in business" was obviously the only person sufficiently qualified to undertake "so good a work." When Franklin replied that in his opinion the present Ministry was not in the least inclined toward conciliation, Barclay took the stand that Franklin was judging the Ministry "too harshly."

[4] Franklin's letter to his son written "On board the Pennsylvania Packet, Captain Osborne, bound to Philadelphia, March 22, 1775" was not published until 1818 when William Temple Franklin included it in his edition of his grandfather's works. The letter is available today in all the standard editions of Franklin's writings (Sparks, Bigelow, Smyth), and is perhaps most readily consulted in Carl Van Doren's one volume selection, *op. cit.* Francis Wharton used the letter as his first selection in his *Revolutionary diplomatic correspondence of the United States* (see References).

Some of these men, he fancied, "would be very glad to get out of their embarrassments on any terms, saving only the honour and dignity of the Government."

In the report which Franklin has given of Mistress Howe's conversation with him after their early games of chess, we find strange echoes of these opinions and suggestions of Barclay's. This similarity leads to surmise. Can it be, we wonder, that some one of this group of influential London people figuring in Franklin's narrative, who are deeply concerned over the American difficulties, had briefed Mistress Howe to suggest to Franklin when sociably inclined by the charm of her company, that he was the one person who could influence his countrymen in the colonies to come to terms with Britain without recourse to war? Franklin has stated that at the time of occurrence he considered the remarks made by Mistress Howe —presumably meaning their resemblance to Barclay's— as merely "accidental." It is Franklin's use of the word *accidental* which almost transforms our original surmise into actual belief that Mistress Howe had been coached to play a part.[5] That very evening, moreover (as if to prove our point that a group of influential people were at work upon a scheme), arguments of the same nature were presented to Franklin when he went by invitation to Dr. Fothergill's house "to confer on American affairs." Notes from both David Barclay and Dr. Fothergill had urged him to take part in this conference. As soon as the three had assembled, Dr. Fothergill asked Franklin to draw up a plan to be presented at their next meeting in terms which might be deemed reasonable by the three of them to use as a basis to avert the war which was threatening. "Such a plan might be communicated," Dr. Fothergill said, either by himself or by Barclay "to some of the more moderate among the Ministers who would consider it with attention"—surely if presented by their trusted friends. Thus the *Hints*—the seventeen points aforementioned—came into being, and Franklin found himself committed to work actively as an American with two British colleagues on a definite plan to achieve reconciliation between colonies and mother country.

From the time that the *Hints* took shape upon paper, the plot began to thicken. When Franklin finished his reading of the document at the December meeting described, Dr. Fothergill immediately proposed showing the propositions to Lord Dartmouth, who was Secretary of State for the North American colonies. Dr. Fothergill had long been Lord Dartmouth's physician and was then visiting his noble patient daily, as it happened. Aside from his official position Lord Dartmouth was known for the breadth of his humane interests. He was an art lover and the patron of artists; a man of piety whom the poet William Cowper was to celebrate as "one who wears a coronet and prays"; he was the friend of George Whitefield and Selina, the Countess of Huntingdon, whose religious principles he had adopted. In America he was fostering with his support a college for the education of colonial youth—a college which still flourishes today in New Hampshire, still proudly bearing the name of Dartmouth. It seemed obvious to the three negotiators that if a man of Lord Dartmouth's prominence should approve of the propositions advanced by the *Hints*, the peacemakers might hope for the success of their plan through the blessing of his official influence with both the prime minister and the King. Lord Dartmouth's personal relationship with the prime minister, Lord North, was uncommonly close, for as boys they had been members of the same household. While both of them were small children, Lord North's father after two years of loneliness as a widower had married Lord Dartmouth's widowed mother. As young men North and Dartmouth had been companions at Trinity College, Oxford, and later had travelled extensively together on the Continent before entering political life.

The nomination of Lord Dartmouth by Dr. Fothergill was followed by David Barclay's suggestion of Lord Hyde as another valuable counsellor. Lord Hyde, then Chancellor of the Duchy of Lancaster, had a long record of public service to his credit, dating from diplomatic missions in his youth to the continent, where he had won esteem from Frederick the Great, and later the decoration of the Prussian Eagle. Barclay venerated him as a man of good will and as an experienced elder statesman personally favored by both prime minister and the King. "He is a very knowing man," Barclay assured Franklin, "and though he is not in the Ministry, properly speaking, he is a good deal attended to by them. I have some acquaintance with him; we converse freely sometimes; and if we were to talk things over, and I should com-

[5] Franklin also stated in his letter, "I had not the least apprehension that any political business could have any connection with this new acquaintance." Van Doren, *op. cit.*, 351.

municate our conversations upon them, some good might arise." No objection to Lord Hyde's qualifications could possibly be raised by either Franklin or Fothergill.[6]

Both Barclay and Fothergill were eager to reveal that Franklin was the author of the *Hints* when they showed the document to their noble friends. This Franklin refused to permit. If they wanted any attention paid to the *Hints*, he told them, it would be better *not* to mention his name, since he had reason to believe that the Ministry was prejudiced against him. It was agreed accordingly that Franklin's name should be suppressed in conversations with Lord Dartmouth and Lord Hyde. As for Franklin, he has asserted in his narrative that he kept proceedings of the meetings in Harpur Street a profound secret.

David Barclay proposed to copy the *Hints* in his own handwriting for Dr. Fothergill's use. This would be a necessary precaution, we can see, under the circumstances. Franklin sometimes found it essential to write to Lord Dartmouth about colonial affairs of concern to both of them. If Dr. Fothergill should show Lord Dartmouth the *Hints* in Franklin's original draft, Dartmouth would undoubtedly recognize the handwriting as that of his correspondent, the colonial agent. In order to keep the secret of Franklin's authorship perfectly intact, Barclay would also prepare a second copy of the *Hints* which would eventually be presented to Lord Hyde for perusal. Dr. Fothergill and Barclay thus took full responsibility for the subject matter of the *Hints*. From such clues as these, laid bare for interpretation by careful readers of Franklin's account, we may conclude that neither Lord Dartmouth nor Lord Hyde had any previous knowledge that this plan to thwart the threat of war had been conceived and was taking shape in Harpur Street under direction of Dr. Fothergill and David Barclay, with the help of Benjamin Franklin. We emphasize this point because credit has been given elsewhere by certain writers to Lord Hyde and Lord Dartmouth for starting the negotiations. Is it likely that loyal ministers of the Crown, who had been required to put aside personal desires (and personal convictions perhaps in some cases) when taking oath of office, would condescend to lay aside the objectivity of their officialdom and instigate such a scheme of negotiation as this which they must know could never win, at its inception, either parliamentary attention or royal sanction? It is equally difficult to believe that plain, straightforward Quakers, such as John Fothergill and David Barclay, would have indulged in a double game of go-between to blind both their American colleague and the noblemen whose counsels they valued as to what contribution each was making toward the success of these difficult manoeuvres. Franklin had agreed that the noblemen were to be consulted, and in due time Lord Dartmouth and Lord Hyde would be informed as to Franklin's share in the business which the two Quakers were ardently promoting.

Two twentieth-century authors, each of whom has retold the story of the negotiations undertaken in London during the winter of 1774–1775 with the hope of preventing the outbreak of war, have interpreted the situation quite differently, and we must briefly consider their views. In 1919 R. Hingston Fox published a thorough-going study of John Fothergill. In this book, *Dr. John Fothergill and His Friends* by title, Fox states that as early as November, 1774, Lord Hyde had met David Barclay and asked him to get Benjamin Franklin to use his influence in America to stop the war which was threatening. Fox has made this statement without reference to any source of authority. Franklin, in his eye-witness account of the negotiations, has made no reference to Lord Hyde until after that second December meeting, held by the committee of three in Harpur Street, when, as we have mentioned, Barclay proposed Lord Hyde, that "knowing man," as one who should read and comment upon the *Hints* because of his supposed influence with the Ministry.

The second author to whom reference is now made is the late Carl Van Doren, whose Pulitzer Prize-winning biography of Benjamin Franklin, a fascinating book, appeared in 1938. Van Doren, possibly following the lead of Fox, whose volume he has listed in his bibliography, has declared: "The actual first suggestion may have come from Hyde to Barclay, and Barclay, as he told Franklin, may have taken it to Fothergill." We note the extreme hesitancy of this statement. No documentary evidence contemporary with Franklin's letter recording the negotiations to prevent war has yet been found for this tentative assumption of Van Doren nor for the positive statement by Fox, that Lord Hyde first proposed Franklin as the

[6] When Lord Hyde, then Mr. Thomas Villiers, refused in 1748 to vote in a controverted election because he could not understand the issues, Horace Walpole regarded him as "a very silly fellow." *The letters of Horace Walpole*, ed. Cunningham, 2: 140, London, 1841.

key man to employ for negotiations with the colonies. Van Doren's theory that Barclay "may have taken" the suggestion of Franklin's name to Dr. Fothergill is based upon the contents of a note to which brief reference has already been made and which is quoted in full by Franklin in his narrative. In this note, written by Barclay, Franklin is informed that after their talk the day before, Barclay had met Dr. Fothergill "unexpectedly," as he said, and had confided to him "the subject of our discourse." We shall recall here and re-examine (for its significance in the theory to be advanced by the present writers) the fact that Barclay before sending this note, had spent an afternoon in Franklin's company working out with him the details of a merchants' petition, designed for parliamentary presentation, which sought to lighten restrictions lately placed upon colonial trade. When this important business was over Barclay had made some flattering remarks to Franklin, as we have previously related, stressing his belief that Franklin had it in his power to mould American opinion into a more favorable attitude toward Great Britain. This was the very same view that Mistress Howe had advanced after her chess games with Franklin, as has also been noted. Franklin had obviously liked these compliments and seemed willing to consider what he could attempt. When Dr. Fothergill heard this report from Barclay he had evidently decided to capitalize upon Franklin's favorable attitude. He had, accordingly, invited both Barclay and Franklin to his house to talk over the situation the next day. A personal invitation to Franklin from Dr. Fothergill had been included with the note which Barclay had written to his American friend. Barclay himself was delighted by the turn of events since the conclusion of his conference on mercantile affairs with Franklin; he would go to Dr. Fothergill's house without fail; and he urged Franklin to attend "a further conference on this momentous affair"—which is, of course, in its widest application, a reference to the entire dispute between America and Great Britain then absorbing the attention of all thoughtful men.

The first meeting in Harpur Street seems thus to have been hastily planned. It may be, however, that if Barclay had not had the good fortune to meet Dr. Fothergill on the street "unexpectedly," as he has said, he would have made an opportunity to see him, in order to report what had happened at the conference with Franklin about the merchants' petition. Perhaps, Barclay, who as a business man was particularly interested in commercial adjustments benefiting trade from the standpoint of the merchants' group to which he belonged, was also acting as Dr. Fothergill's scout to capture Franklin's influence when the time was ripe for work upon a new phase of this extremely complex "momentous affair." Certainly the conference in Harpur Street turned at its very start under Dr. Fothergill's chairmanship specifically in a new direction. The problem enlarging in scope became how to procure settlement of all misunderstandings by some mutual agreement which should secure restoration of peace and harmony in both countries. It is not improbable that Dr. Fothergill, who acted as host and leader at these conferences which had their beginning in his study, was also the instigator of the whole plan of discussion of this question at a conference table, where each country could have representation, and where he hoped a plan of reconciliation might eventually be shaped. What more likely than that London's foremost Quaker leader, who was also London's most prominent physician dedicated to the service of mankind, should have originated these conferences to prevent the war which all feared was approaching?

A curious letter in the collections of the Historical Society of Pennsylvania may here find mention. Written by Dr. Fothergill eight years previously in 1766, probably to a professional friend, Dr. Lionel Chalmers, residing in Charleston, South Carolina, it discloses how long an obsession against the barbarity of war had possessed the Quaker doctor's mind. Quite mistakenly, in our opinion, he has placed the blame for the glorification of war upon the teachings of classical writers. "Unfortunately for us," he has quaintly put his argument,

> we are introduced in the whole course of our education into War, Battles, Sieges, Carnage, and no Character is thought to be equal to that of a Hero—a licensed, wholesale Murderer. We are taught to be in love with Virgil's heroes, with Homer's—and in sober prose with the lives of men who have made themselves illustrious from Slaughter. Multitudes never emancipate themselves from these obsessions. How much happier are those who have no Learned Prejudices to eradicate!

It is the peace-loving Quaker whom we hear enunciating a doctrine developed from the noncombatant principles of his religion as a member of the Society of Friends.

Fortunately, for the serious support of our

view as to Dr. Fothergill's leadership as peacemaker at the conference table established in the interest of averting wartare between Great Britain and the American Colonies, we have been able to gather information from primary sources both in England and in America. At Patshull House, in Staffordshire near Wolverhampton, the present Earl of Dartmouth has in his possession an important collection of American papers which were flowing in constant stream to the desk of his ancestor, the second Earl of Dartmouth, when the latter was Secretary of State for the North American colonies from 1772 to 1775. These ancestral papers have been very kindly put at our disposal by the present Earl of Dartmouth. Equally valuable for the investigation undertaken at this time is an American collection of letters written by John Fothergill to Quakers in Philadelphia and to other important people throughout the colonies. These letters cover the wide field of their writer's interests—religion, politics, medicine, human relationships. They deal with questions of provincial government; with medical students sent to Great Britain to study in London and Edinburgh; with the necessity for the establishment of medical schools and hospitals in a pioneer country; with projects for the collection of natural history specimens, particularly of plants to be exported from America for Dr. Fothergill's garden in Upton; with the exploration of wilderness territory in Georgia and Florida. The Historical Society of Pennsylvania has appropriately fallen heir to this wealth of material, in which, among many other topics, the continuous association of John Fothergill and Benjamin Franklin may be traced. We must review briefly, at this point, the story of John Fothergill's long connection with the American colonies, in order to comprehend the development of his interest and understanding of colonial conditions, which led finally to his active sponsorship of the negotiations to prevent the War of Independence.

From letters consulted in Philadelphia we learn that in 1742, when John Fothergill, as a young man of thirty, was just becoming established as a doctor in London, he was asked to accept the duties of official correspondent to the Philadelphia Meeting, then the leading organization of the Society of Friends in Pennsylvania. To become, as he phrased it, "a canal of communication" between England and America seemed to him a privilege which would extend the horizon of his interests. He considered the appointment a tribute to his deceased father, John Fothergill, Senior, who had made three trips to America earlier in the century as an itinerant Quaker missionery. John Fothergill the physician, whose career we are now considering, was soon to prove useful outside the fields of medical and religious endeavors. When Thomas Penn, his Lady Juliana and their children became his patients Dr. Fothergill found himself the confidant of Pennsylvania's absentee Proprietor. From this connection, which placed him in a strategic position to sound out public opinion on both sides of the water, we may trace the evolution of Fothergill's "political character," later ascribed to him by his nephew, William Hird of Leeds, who became his eulogist. Pennsylvania's early history was full of hardship and struggle for its people, governed as they were at the time by an absent Proprietor, beset at home by Indian warfare, and hampered by non-combatant Quakers as a majority in the Assembly. Dr. Fothergill, a shrewd observer, counselled leaving to Caesar the things that are Caesar's; and the government of Pennsylvania was perhaps more manageable, if less idealistic, when in the course of time Quaker influence declined.

As the years passed John Fothergill gained conviction and courage. In 1765 he opposed the Stamp Act as unwarranted taxation upon the American colonies. In his letters written between 1765 and 1775—a decade when prevailing conditions approached on a smaller scale the state we now call *cold war*—he took a firm stand for unhampered American development, predicated to a large extent upon Great Britain's economic needs which were mirrored in the colonies. The flow of exports and imports was already becoming imperilled by the resentment of colonists who considered themselves unjustly taxed and were retaliating by backing legislation for non-importation of British goods. In the flood of pamphlets occasioned by passage of the Stamp Act, John Fothergill's *Considerations Relative to the North American Colonies* remains outstanding, both for content and for organization of argument. Though it reveals his romantic persuasion that America is a potential Utopia, it is crowded with facts about the new country. He has stressed American diversity; he has shown a surprising knowledge of the differences to be found between the farmers and small tradesmen in the north and the wealthy planters in the south with their slaves; he has emphasized the increasing values these

colonies will create through their productive power as an extension of Great Britain's resources.

When the Stamp Act was repealed by Parliament in 1766, Dr. Fothergill had an unexpected opportunity to defend Benjamin Franklin against a charge emanating from Pennsylvania that Franklin had been playing his countrymen false and had been supporting the additional taxation favored by this insidious act. Had not Franklin at first suggested "discreet and reputable persons" whom the British government might employ as stamp officers in the colonies? Fothergill was able to clear Franklin's reputation by sending a statement to Philadelphia for publication in the newspapers. He gave Franklin generous praise for "unswerving attachment" to American interests. When Franklin stood at the bar of the House of Commons, he had informed the Ministry of the "mischievous tendency" of the Stamp Act, which he had "uniformly opposed to the utmost of his ability." Fothergill could do this with good grace. He had been Franklin's physician since 1758, the year after Franklin's arrival in London for a stay of five years as Pennsylvania's accredited agent. It was in fact Dr. Fothergill who first introduced Franklin to Thomas Penn with whom the agent was to have endless disagreements over Pennsylvania's proprietary government. Indeed we might almost claim that it was Dr. Fothergill who discovered Franklin as a scientist; at least, even before meeting him, Dr. Fothergill was able to detect the authentic note of distinction in Franklin's early account of his electrical experiments which had been reported from Pennsylvania by the investigator in a series of letters to Peter Collinson. Fothergill apparently exerted himself to have these letters published by Edward Cave in 1751;[7] at least he wrote a foreword to introduce the little book, hardly more than a pamphlet; and this must have carried weight with English readers, who first made acquaintance in its pages with an unknown American scientific worker whose later contributions would win the laurels of publication in the *Transactions of the Royal Society*.

[7] Dr. Fothergill thought Franklin's communications "of too much value to be stifled and advised the printing of them." Collinson then gave them to Cave to use in his *Gentleman's Magazine*. Cave preferred to issue a pamphlet in 1751, which "by the additions that arrived afterwards swelled to a Quarto which has had five editions and cost him nothing." By the time of the American Revolution the book had been translated into French, German and Italian. Farrand, Max (ed.), *Benjamin Franklin's memoirs* (parallel text edition), 382, Berkeley and Los Angeles, Univ. of Calif. Press.

Twenty years acquaintance had thus brought its fruitage of perfect trust when John Fothergill and Benjamin Franklin sat together, as aging men, at a conference table in Harpur Street during the winter of 1774–1775. David Barclay proved of great use to them at this time. As the youngest of the three, he seems to have done most of the running about on business for the group. He acted apparently as secretary for the triumvirate. It was he who sent out notices of the meeting; it was he who copied the *Hints* and repeatedly interviewed Lord Hyde who loved to talk—but wrote few letters for investigating historians to read. Barclay finally took responsibility for putting the modified *Hints* on paper, discarding Franklin's title for one of his own so that the new propositions (though very few changes had actually been made) blossomed forth on February 16, 1775, as *A Plan Which 'Tis Believed Would Produce A Permanent Union Between Great Britain and Her Colonies*.

Medical men have little time in the twentieth century for active participation in political undertakings on an international scale. How was it with John Fothergill whose career we are now considering? Medicine was less developed in the eighteenth century, but patients were much the same in their demands for a physician's attention, and it took a doctor much longer to make his rounds by coach. Dr. Fothergill's sister Ann, who kept house for her unmarried brother, was always complaining to her correspondents that he was overworking in his practice. His own letters bear this out. Benjamin Franklin once wrote to ask when the doctor expected to begin to live his own life—to *live*, as he cynically expressed it. Perhaps because the medical profession has always exacted intense concentration from its practitioners, Dr. Fothergill's important part in these negotiations now under study has been overlooked. Was he really anything more than a dignified figurehead for the work undertaken, a careful chairman of committee? Van Doren has coupled Dr. Fothergill's name rather strangely with Lord Dartmouth's by saying: "Whether Dartmouth or Fothergill suggested it, Fothergill had been authorized—or permitted—to consult with Franklin about some plan of reconciliation with the colonies." Although Van Doren has already admitted that the beginnings of the plan were "somewhat mysterious," and also that "if he wanted Franklin's help, there was no reason why Dartmouth should not have gone directly to Franklin,"

he has concluded his diagnosis of the situation by stating, "Great lords could not chance a rebuff. They preferred to work indirectly through Fothergill and Barclay, who as Quakers could hold no public office and who as Englishmen (or at least Scots) were friends of America and of Franklin." Study of John Fothergill's letters as they exist today in England and America brings new light to bear upon these personal relationships. The letters at Patshull House, though only a handful of John Fothergill's are there, show that Lord Dartmouth's physician was such a trusted friend of his lordship that it was not Lord Dartmouth who supplied Dr. Fothergill with his ideas about America but Dr. Fothergill who advised Lord Dartmouth how to deal with some of the most troublesome American problems. In regard to the statement by Van Doren previously quoted that Dartmouth "authorized—or permitted" Fothergill to talk Franklin into taking part in a scheme to achieve reconciliation between the two countries, such a conception seems completely untenable, if not absurd. If this had been the case, Franklin's name would never have been suppressed as author of the *Hints* when a copy of these propositions, disguised by Barclay's handwriting, was handed by Dr. Fothergill to Lord Dartmouth. At the time when Franklin demurred as to the use of his name in the matter, because he believed there was ministerial prejudice against him, Dr. Fothergill would have replied, "Ah, but you are wrong, Dr. Franklin. Lord Dartmouth, himself, has asked us to get you to make us a satisfactory plan to secure peaceful relations again with the colonies."

In passing we must also note one small palpable error of fact which has been made when both Barclay and Fothergill are listed among the Scots in Van Doren's account. Barclay of course gloried in his Scottish ancestry as grandson of Robert Barclay of Urie. Robert Barclay's famous book, *The Apology for the true Christian Divinity as the same is held forth and preached by the people called in scorn Quakers*, had become as authoritative to members of the Society of Friends as the catechism is to certain other sects. John Fothergill, however, was no Scot but a Yorkshire man, born in Wensleydale, 12 March 1712, perhaps a descendant of Sir Robert Fothergill who served as general under William the Conqueror when he took York in 1068. The only connection with Scotland of which John Fothergill could boast was his good fortune in having enjoyed tutelage by the eminent medical faculty in the University of Edinburgh, where he received his medical degree in 1736 at the age of twenty-four.

At Patshull House among the Dartmouth papers, a letter has been found from Dr. Fothergill to Lord Dartmouth which earlier attracted special attention from B. F. Stevens when making his digests of the Dartmouth MSS. in 1895, as a report for the Historical Manuscripts Commission. This letter seemed curiously interesting to Stevens "from its early suggestion, 29 August 1765" (as noted on its wrapper in an eighteenth-century hand) "of Commissioners being chosen to go to America to confer with others appointed there on the subject of their complaints, intimating that such a conference would give a correct knowledge of the Americans and avert the independence they seem to desire." This terse notation, the work of Stevens, well summarizes the letter.

When the actual letter written by Dr. Fothergill is examined we find that when writing the superscription he has neglected to insert the year of the letter's composition. Since the letter itself is couched in general terms it is difficult to determine the year of its writing from context alone. In its pages Dr. Fothergill has referred to "some late acts and regulations which have spread universal discontent and produced some very unjustifiable proceedings on the other side." You will notice that he does not refer to a single act, such as the Stamp Act of 1765, which one might suppose from the use of this date—1765—on the wrapper would occur. Since we find this to be the case we are led to wonder if by any chance the "late acts and regulations" which he mentions are not references to the so-called "intolerable acts," about which Massachusetts and other colonies were bitterly complaining almost a decade later, before the outbreak of the War of Independence in 1775. Can it be that when this letter was prepared for safe-keeping, probably by some eighteenth-century secretary in Lord Dartmouth's colonial office, 1765 was hastily written on the wrapper of this letter by mistake? Who can tell? The concept of independency arose in America much earlier than the outbreak of the War of Independence in 1775, but this idea did not reach the height of its acceptance by revolutionary leaders until the closing of the port of Boston in March, 1774, followed in quick succession by other restrictive measures, had aroused a sympathetic attitude throughout the colonies toward what was considered the unjust punishment of a sister colony for rebellious acts,

most notably the so-called Boston tea-party on December 16, 1773, and subsequent demonstrations.

Whatever its date, the significance of this letter remains two-fold. In the first place Dr. Fothergill has ventured to give advice to Lord Dartmouth about the troublesome American question. He has approached the matter delicately. "A circumstance has occurred to my thoughts," he wrote "which though out of my sphere seemed not unworthy of attention." He has admitted that he would . . . "need . . . all my Honourable Friend's partiality to excuse me for venturing upon subjects so much above my reach." He has then proceeded to outline a definite plan of action —which was a step farther for him in his daring, indicating the growth of his influence with Lord Dartmouth. Fothergill's plan, as Stevens has already informed us, was to send commissioners to America to confer with colonial commissioners appointed by their respective governments upon the subjects of their complaints.

This was a idea which was suggesting itself to other minds. From Franklin's narrative we learn that it finally cropped up in Grafton Street where Franklin still went occasionally for a game of chess with Mistress Howe. On Christmas Day, 1775, Franklin had been introduced to the lady's brother Lord Howe, the Rear-Admiral. It soon turned out that Lord Hyde had been talking to Lord Howe. After the *Hints* were shown to chosen advisers the secret of the negotiations undertaken had been hard to keep. Knowing now of the plan on foot to prevent hostilities, Lord Howe offered his services as intermediary for talks with Lord Dartmouth and Lord Hyde, and one day after talking to Lord Dartmouth he came to Franklin to ask his opinion about sending commissioners to America. Mistress Howe who was present at the time, at once suggested that this brother of hers was the very person to send, in company with Dr. Franklin. The Howes had maintained a special link with America ever since the loss of their brother George, distinguished for bravery, who had fallen at Ticonderoga in 1758 during the Seven Years War. In recognition of his valor the people of Massachusetts had raised funds to memorialize him in Westminster Abbey. No wonder the Howes had, as Franklin phrased it, "a peculiar regard for New England which had shown a particular respect for their family." It seems very likely that the idea of sending commissioners to America, strongly advocated at this time by Lord Howe, had been the subject of conversation between him and Lord Dartmouth. This was the very plan which had been much earlier suggested to Dartmouth by Dr. Fothergill. Coming from Dartmouth to Lord Howe and then to Franklin, who would certainly speak of it to Dr. Fothergill, the idea would have travelled full circle back to its originator in this influential group of Londoners.

Discussion at a conference table was Dr. Fothergill's favorite prescription to cure political misunderstanding. To his American friend, James Pemberton of Philadelphia, he would soon present the very reverse of the plan which he had offered to Lord Dartmouth. In a letter, clearly dated 23 August 1774, studied in the Historical Society of Pennsylvania, we find Dr. Fothergill proposing that "a few sensible sedate persons" chosen by their respective governments should come over immediately from America to London (we must remember the voyage would take at least six or seven weeks) for conferences with British leaders. The American representatives should seek interviews, individually, so far as possible, with members of both Houses of Parliament, and "with great moderation yet proper firmness," Fothergill has counselled, "give them full information of your situation, apprize them of the state of the dispute between you . . . , and urge their attention to the mutual and reciprocal interest of a great Empire." English Friends—Quakers, that is to say—and the colonial agents resident in London would gladly make contacts for the American visitors with important British leaders. The leaven of peacemaking would spread, Dr. Fothergill believed, when each side learned that the other wished for friendly relationships, but he has taken pains to admonish Pemberton as to the correct attitude.

> You must entreat for that which you have a *right* but not the *power* to demand. Whatever mode of prudence is decided, if something of this nature is not adopted, both you and we are, I fear, on the brink of a fatal precipice. Petitions, memorials, addresses, with whatever cogency of reason they are composed, will be totally inadequate.

At this critical time for making decisions John Fothergill's plan for sending commissioners one way or the other across the Atlantic was never attempted. The only conference on colonial differences which he was able to help in arranging took place in his own study. We can well understand without any stretch of imagination what satisfaction these meetings brought Dr. Fothergill at their start while hopes were high. American representation had been secured in the person of

Benjamin Franklin who fully understood the British position from his many years of residence in London. It was hoped that his statement of the colonial case would win attention and support eventually from Lord Dartmouth, the Englishman best fitted to understand America's needs from the nature of his post in the Ministry as colonial secretary.

Dr. Fothergill did not of course invent the idea of discussions by travelling commissioners as a way to prevent war. It was an idea as old as ancient history. It was also afloat at this time in the colonies. We find it solemnly propounded by Joseph Galloway, long prominent in Pennsylvania politics. In a letter to Franklin's son William, then Governor of New Jersey, Galloway recommended such a plan in the present crisis "as a Mode pursued by the Roman, Grecian and Macedonian Colonials on every occasion of like Nature." Galloway's recommendation of discussion by commissioners was destined to perish on paper, and a like fate awaited a more serious effort of his, a plan of union with Great Britain presented in good faith to the Continental Congress, a plan which would advocate as its main features a President General with a Grand Council in America as an inferior and distinct branch of the British legislature, united and incorporated with it, the consent of both [English and American] branches requisite to passage of all general acts and statutes. Galloway's sponsorship of something akin to Dominion status for the Colonies alienated Congress; his plan was expunged from its records; and he eventually sought exile in England where he lies buried.

During this period of tension a letter, intercepted as it crossed the Atlantic after the outbreak of war, and now preserved in the archives of the Home Office, was addressed to John Wesley by a pious Methodist in New Jersey, imploring Wesley to induce Lord Dartmouth himself to come to America to settle in person the quarrels threatening to develop into a disastrous war.[8]

In Harpur Street, meantime, hopes of reconciliation had died after much running to and fro, many notes, letters, interviews, meetings, and conversations of infinite detail. The noble lords, who were acting as background consultants, had always considered the *Hints* "too high" in their demands. The majority of the propositions seemed "inadmissible," in their opinion, especially if intended for Parliamentary consideration. As for Franklin, he had no authority to drive a bargain. Although he had once offered to pay personally for the tea thrown into Boston Harbor during the Boston tea-party, he would do so only if the so-called "intolerable acts" were rescinded. The time for bargaining had passed, alas. The conferences were ending shortly before the news reached London that Franklin's wife had died in Philadelphia. He must return home as soon as possible. On the eve of his departure from London Franklin received a farewell message from Dr. Fothergill. In this note, the original of which may be read today in the Stevens manuscripts in the Library of Congress, Dr. Fothergill urged Franklin to explain the situation frankly to their friends when he reached home. The Philadelphians were to be told that

> Whatever specious Pretences are offered, they are all hollow, and that to get a larger Field on which to batten a Herd of worthless Parasites is all that is regarded. Perhaps it may be proper to acquaint them with D. B.'s and our united Endeavours and its Effects. They will stun, at least, if not convince, the most worthy, that nothing very favourable is intended, if more unfavourable Articles cannot be obtained.

Before Franklin could deliver this message war had actually broken out in America. While he was homeward bound as a passenger at sea the inevitable clash came between colonists and British troops sent out from Boston to Concord, Massachusetts, to capture stores of ammunition secreted in country hideouts for use by rebellious Minute Men. After the fighting started 19 April 1775 in Lexington and Concord, peace was a lost cause.

In England, however, in spite of the outbreak of hostilities, the question of sending peace commissioners to America was brought into the open as a subject of deep concern to Parliament, where it was kept so long in dispute that it was not until May, 1776, that delegates were actually chosen for this mission. The choice fell upon the Howe brothers, Lord Howe and Sir William, long known for their American sympathies. Sir William was already in America, where, as successor to General Gage, he had been resident long enough to win distinction as leader of British troops in the battle of Bunker Hill, June 17, 1775. Lord Howe,

[8] Letter from Peter Shiras of Mount Holly, Burlington Co., New Jersey (Shiras family emigrating from Scotland settled in Mount Holly, 1750) to Rev. Mr. John Wesley. Dated Oct. 29 [1775], this letter is one of a series seized in transit, opened at the General Post Office and sent to the Secretary of State. *Calendar of Home Office Papers of the Reign of George III* 1: 452, 453, London, 1878-1899.

bearing the olive branch, would cross the Atlantic to join Sir William in New York. Together they would treat for terms of adjustment with colonial representatives appointed by Congress. It will be realized that Lord Howe's appearance as peacemaker in New York in September, 1776, two months after the issue of the Declaration of Independence in July, placed him in a curiously awkward position. Although he bore the olive branch of peace, symbolically, he was also the newly appointed commander of a formidable British fleet with vessels at anchor in New York Harbor. British troops numbering 30,000 men were at the time engaged as ground forces in colonial warfare, and Sir William Howe was their commander-in-chief. Reinforcements for his army were crossing the Atlantic at the very time of Lord Howe's passage. It was, you will agree, a strangely impossible situation. Could men of such temperaments as possessed by Benjamin Franklin and John Adams, who met the Howes at Staten Island, listen to propositions of pardon upon submission, when presented to them by the military leaders of a country with which they were actually at war? The time for bargaining had passed, alas. The fatal precipice of Fothergill's prophecy had been reached. No paper plans, whenever they arrived, whatever their cogency of argument, could save the situation at this juncture.

Conflicting ideologies had produced that tragic predicament described movingly by Herbert Butterfield of Cambridge University in his wise book, *History and Human Relations*. To borrow his words, in situations of this nature, a terrible deadlock of conflicting opinions has created an irreducible dilemma. Two nations find themselves pitted against each other, each desperately anxious, it may well be, to avoid destructive conflict, yet each desperately resolved to make no compromise of principle, each desperately fearful of what the other may do. The result in the case we have been studying was inevitably war.

In conclusion let us give historical imagination full sway; let us look again at an eighteenth-century scene; let us go to St. James's Square to pay our respects to Lord Dartmouth in the winter of 1775, while the *Hints* are still under consideration. Since the afternoon is waning we can spend only a moment with the Secretary of State for the North American Colonies. We shall discover him to be a man of great personal dignity and charm—the characteristics Thomas Gainsborough caught when painting his portrait. We shall find him studying with sober face a letter which seems to absorb his complete attention.

It is this letter, a communication written by John Fothergill while the negotiations to prevent war were under way, and now preserved among the official papers at Patshull House, which is presented as final evidence of Dr. John Fothergill's importance as a peacemaker.[9]

Harpur Street
12th Instant.

My Noble Friend,

I scarcely know how to attempt to write, nor yet am wholly at ease to be silent on the present interesting situation of this kingdom.

It may seem vain for a private individual to offer his opinion in a case of so great consequence, and especially in an affair so remote from his situation.

Permit me to mention a few of the circumstances which have led me to be acquainted with America; if they appear of any moment I know they will have weight with the Nobleman I am addressing, both from the partial regard he is pleased to pay me, as well as from the circumstances themselves.

My Father, a man of the middle rank of life; plain, simple, judicious, visited America three times in the course of his life—all the inhabited parts, and observed the progress of population at very different periods.

A Brother, whom I lost a few years ago, likewise passed through the continent not many years since.

Some others of my relations, many of my acquaintances of whose good sense and probity I could not admit of a doubt, have likewise at different periods seen the whole country.

I have corresponded with some of the most sensible people in most of the Colonies for upwards of thirty years on different subjects, with men of different professions; and have been occasionally even visited by them (over) the like period.

With some little inquisitiveness, some little knowledge of the general history of the globe, some acquaintance with what passes commonly in the world, a very mean capacity must have unavoidably acquired, not from system but from accident, perhaps, a general idea of the coutry, its inhabitants, its connections, its prospects—Such is the little stock upon which I presume to form any ideas of this most important object.

America is now grown too great to be humbled by this country, without such an exertion of force as might—like what happens in the human frame too frequently—bring on such an Hemoptic—as would end in a fatal consumption.

We might harass, even destroy a large territory, many people, retard their progress toward greatness, but never, never hope to subject them to Great

[9] The dating of this letter is incomplete; month and year are missing from the original manuscript.

Britain. This is at least my firm belief, and founded on what appears to me indubitable evidence.

What, then remains to be done? The *Hints* we have offered (D.B. and myself) would secure their obedience and our Superiority—Grant them rather more than less—Make it evidently their Interest to be our Friends to allow us an exclusive trade—to consider us, as they have done and always will do (if we act our part as we ought) as their Parents, Guardians and Friends, and everything will soon return to Peace and Harmony.

It requires a large capacious heart to take on the consequences of disunion with such a country—This little Island will soon become the prey of any bold invader, the moment we are separated from our Colonys—Where are resources against France, against Spain—much less against both United?—view our debts, consider our inability to feed ourselves with bread; see France and her colonies united—Spain and her distant possession one empire. What can Great Britain and Ireland oppose to such powers? Do, my Noble, much-esteemed Friend, forget the little, trifling quarrels, fomented by mischievous people for the ruin of this great Empire, and give America all she asks—was my life worth pledging I think I could do it safely, that she will amply repay the condescension. Violent measures will ruin us both—partial condescension will pave the way to future and worse misunderstandings—a generous mind will open itself to the liberal plan of human felicity and regulate its actions accordingly.

Excuse this hasty effusion of a mind ardently wishing for the happiness and security of mankind in general, and not less for the honour of Great Britain and of those great names who are now permitted to be in such a situation as to decide upon this nation's prosperity and their honour and reputation forever.

I am,
Lord Dartmouth's obliged,
respectful Friend,
JOHN FOTHERGILL.

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THE SOURCES OF EVIL ACCORDING TO PLATO

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THE point of this paper is the plural in its title. My reason for making it is the persistent failure of scholars to understand how Plato, if he had a consistent theory of evil, could speak of the evil in this world as derivative from more than a single source. Some of them, consequently, stoutly maintain that he must have believed all evil to have one source only, though they disagree in identifying that source, while others deny that he ever achieved any coherent theory of evil at all.[1]

This is not so strange as a similar lack of consensus concerning evidence would be in most fields of investigation. Plato did not collect all that he might have to say about various subjects and set down these doctrinal opinions in systematic arrangement under the rubrics that later became the conventional problems of philosophy. The form in which he chose to express his thought is the dialogue, and he always confined his treatment of any problem to such aspects of it as seemed to him to be pertinent to the particular context of the philosophical discussion which he was actually composing.[2] Because of this the interpretation of Plato is involved in peculiar difficulties, and there is a great temptation to treat the several dialogues as records of disconnected phases in the flux or—more courteously—in the development of Plato's thought.[3] One should, of course, beware of reading into the text what is not implied by it according to Plato's own standards; but one must be equally wary of neglecting the illumination that the different dialogues cast upon one another. Synoptic understanding is no less necessary in this modest field of investigation than Plato said it is to the philosopher in his larger sphere. Such a synoptic reading of the dialogues reveals, I believe, behind all the apparently diverse statements concerning the sources of evil a theory more complicated than any of the current interpretations has recognized but perfectly coherent in all its parts and consistent with Plato's fundamental theory of reality.

According to that theory the phenomenal world is a spatial reflection of the ideas, which alone are

[1] Among those who maintain that the source of all evil according to Plato is matter or corporeality are Vlastos (1939: 80–82), Festugière (1947: 36–42), and Pétrement (1947): 45–47 and 72–73); among those who contend that it is soul or an irrational element in soul are Wilamowitz-Moellendorff (1919: 320–321), Chilcott (1923: 29–31), Taylor (1928: 117; 1937: 455, n. 2, and 492), Cornford (1937: 209–210), and Morrow (1950: 163). For earlier exponents of the latter position, against which Hoffleit (1937) argued, *cf.* Zeller (1922: 765, n. 5), who for his part (1922: 973, n. 3 and 4) contends that in all dialogues before the *Laws* the source of all evil is matter but in the *Laws* is an evil world-soul and that this was a natural step in the development of Plato's theory. Greene (1944: 301) thinks it idle to seek in Plato for a single solution. According to him there are two solutions equally Platonic: "Plato identifies the source of moral evil at first chiefly with body or matter and then more and more with soul. . . . The unresolved residuum, or evil, in the world, as Plato sees it, may confidently be assigned to matter or 'Necessity,' once and once only . . . conceived as endowed with life (soul)" (*op. cit.*, 311). Sesemann (1912: 180) had already asserted that there are "zwei Grundauffassungen des Bösen, die in dem platonischen System unversöhnt einander gegenüber stehen." Meldrum (1950: 65) holds that "the discrepancies in what Plato says about evil . . . call attention to something obscure, perhaps incoherent, in his metaphysical thinking"; he argues (70) that "Plato's view of evil varies as νοῦς δημιουργικὸς or ψυχὴ ἀρχὴ κινήσεως predominates," and he concludes (74) that "there is no entity that we can call 'Plato's theology.'" Palas (1941: 52) goes even further and asserts that "the problem of evil never seriously concerned Plato at all," an assertion which Solmsen unwittingly contradicts (1942: 142) with the assurance that "the problem of evil . . . had an organic and important place in his thought." For the interpretations of Plato's theory of evil given by Aristotle, Theophrastus, and Eudemus and for the theories of Speusippus and Xenocrates *cf.* Cherniss, 1944: 95–97, n. 62, and 268–269, n. 176.

[2] The body of the *Timaeus* is not a dialogue; but dialogic characteristics are not entirely absent even from this exposition, just as they are present in the longer expository sections of the *Laws* (*cf.* Schaerer, 1938: 153–156). Moreover, the form of the *Timaeus* alone determines the treatment of important doctrinal factors there, e.g. the self-motion of soul (*cf.* Cherniss, 1944: 428–431).

[3] Stenzel (1931: 108, n. 1; 125; 133), who himself sought to trace Plato's development, issued a warning against the dangers of this temptation, a temptation to which British scholars have recently manifested a growing susceptibility.

perfectly real entities.[4] Since no copy or reflection can be identical with its model or original, all phenomena must fall short of the reality of the ideas, and all must therefore be something less than perfect.[5] So all the phenomenal world is always involved in what may be called "negative evil," since it is a derogation of reality, the degree of deviation from the original which at the very least is implied in the existence of a copy or reflection.[6] It is evil in this sense that, as the contrary of good, is in the *Theaetetus* said to be ineradicable from this mortal world but absent from the divine;[7] and the same notion is implied by the statement in the *Philebus* that whatever fair things there are in this world are the result of a combination of the determinate and the indeterminate,[8] for indeterminateness characterizes phenomena as falling short of the complete reality of the ideas.[9]

Despite this negative evil, this deficiency which phenomenal existence as such implies, and despite the admission that for man at least there is more of evil in this world than good,[10] Plato in his myth of the creation declares that the Demiurge fashioned this universe because he was good and desired all things to be as nearly like himself in this respect as possible.[11] This myth expresses in the synthetic form of a cosmogony what is in fact an analysis of the constitutive factors of the universe which for Plato has neither beginning nor end,[12] and in the myth the Demiurge symbolizes the factor of rational causation in this universe. The Demiurge or god or gods or the class of causes so symbolized or represented[13] is wholly good and is responsible only for good;[14] and the cosmos fashioned by this cause is repeatedly declared to be good—good, that is, as a whole and as good as is possible considering the conditions on which it can exist at all.[15] Primary among these conditions is the nature of reflection or copy already mentioned. The Demiurge does not create either the ideas or space. Both of these are uncaused and ultimate factors;[16] and the immediate consequence of their existence, quite apart from any demiurgic causation, is the reflection of the immutable, non-spatial ideas in the unchanging and homogeneous mirror of space.[17] It is these spatial reflections upon which the demiurgic action

[4] *Timaeus* 52 A–C, *Politicus* 285 D9–286 B2 (*cf. Phaedrus* 250 A–C), *Phaedo* 74 D–75 D.

[5] *Cf. Cratylus* 432 C–D, *Sophist* 240 A–B (*cf. Republic* 597 A 4–7).

[6] Palas (1941: 50) says: "Letzten Endes aber beruht der axiologische Grundcharakter des empirischen Kosmos darin, dass der Kosmos das Abbild des Urbildes ist." This statement, which save for the first two words is literally correct, erroneously implies, however, that Plato meant to explain *all* evil in this way. It is Sesemann's similar neglect of Plato's distinctions that accounts for his formulation (1912: 174 and 176): the notion of the spatial-material principle as the source of all imperfection makes evil a positive force, whereas the notion of soul (through its ignorance) as the cause precludes all positive reality of evil, making it mere negation.

[7] *Theaetetus* 176 A 5–8. This statement concerning the contrariety of good and evil was expanded into a theodicy by the Stoics (Aulus Gellius, VII, i, 2–3 [*S.V.F.* II, frag. 1169], where Chrysippus also uses *Phaedo* 60 B–C); and Plotinus (III, ii, 5, 25–32) employs this passage as an appendix to a series of Stoical arguments, though he interprets it in a non-Stoical fashion. Sesemann (1912: 183) makes the passage say that "die Erhaltung des Guten fordert daher auch notwendig die Erhaltung des Bösen," supposes this to mean that evil is not only necessary but morally justified, and reads this conception into the *Lysis*, the *Timaeus*, and the *Laws*. But the ἀνάγκη of *Timaeus* 48 A and the account of *Laws* 904 B–D refer, as will be shown, to quite different matters; and *Lysis* 220 D–221 D is not parallel either, for its subject is the reason for man's desire of the good and it rejects the suggestion that the abolition of evil must involve the abolition of what is not evil (221 B–D). As for *Theaetetus* 176 A 5–8, the evils there in question, since they are said not to exist in the divine world and to be such as the soul can get free of, must be of a kind that is peculiar to phenomena as such, something implied in the nature of phenomenal existence and not in the nature of good itself. Nor is the good to which this evil is here said to be contrary the ideal or absolute good; it is the derivative good in the phenomenal world itself, as is shown by the remark of Theodorus (176 A 3–4) which motivates Socrates' reply. The "expansion" of the passage by the Stoics and by Sesemann is therefore unwarranted, at least as an interpretation of Plato's meaning. (On this "expansion" and the Stoic paradox *cf.* also More [1921: 235]).

[8] *Philebus* 26 B 1–3 and 25 E –26 B generally.

[9] *Cf. Philebus* 16 D7 – E2 and *Amer. Jour. Philol.* 68: 233–234, 1947 with references *ibid.*, 234, n. 71.

[10] *Republic* 379 C 4–5, *Laws* 906 A2 –B3 (*cf.* O. Apelt, *Platons Gesetze* 2: 541, n. 82).

[11] *Timaeus* 29 D7–E3, 30 A 1–2.

[12] *Cf.* Cherniss, 1944: 421–431.

[13] *Cf.* Cherniss, 1944: 607–608 for the nature of the Demiurge in the *Timaeus*, the nature of "cause" in the *Philebus*, and Plato's use of "god" in the singular and plural.

[14] *Timaeus* 29 E1, 30 A6–7; *Republic* 379 B–C (*cf.* 617 E5 and *Timaeus* 42 D3–4); *Laws* 900 D2–3 and 900 E6–8. *Cf.* the class of causes δσαι μετὰ νοῦ καλῶν καὶ ἀγαθῶν δημιουργοί (*Timaeus* 46 E4).

[15] The last sentence of the *Timaeus* (92 C4–9) solemnly declares the goodness of the phenomenal universe. That its goodness is limited, however, by the very conditions of its existence is indicated by the restrictive qualifications in such passages as *Timaeus* 29 A5, 30 B5–6, 46 C8, 48 A2–5, 53 B5–6. That in many cases its partial aspects are good only by reference to the goodness of the whole is most clearly expressed in *Laws* 903 B4–D3.

[16] *Timaeus* 52 A1–4, 52 A8–B2 (*cf.* 51 A4–B2).

[17] *Cf.* Cherniss, 1944: 453–454.

is brought to bear, not to eliminate their character as reflections (for that is essential to their existence) and so not to annul the negative evil implicated in that character but by delimiting and organizing them to bring them nearer to conformity with the ideas, which are at once their originals and the models of this demiurgic activity.

These reflections, however, as consequences of the mere existence of space and the ideas, though they would be confused and indeterminate,[18] would still be static; and what is reduced to order by the Demiurge is said to have been not at rest but in erratic motion.[19] This precosmical chaotic motion of the myth is an isolated factor of the actually existing universe, random disorder which is only for the most part made subservient to the purposes of rational causation, for the phenomenal cosmos is the result of a combination of intelligent causation and "Necessity" or the "errant cause" that limits its effectiveness.[20] This erratic or random motion is, then, a source of evil different from the mere derogation of reality inherent in the nature of spatial reflection. The confusion of the errant cause with space to produce a Platonic matter which of its own nature is in disorderly motion is the chief reason why many interpreters have contended that in the *Timaeus* at least matter is the source of all evil.[21] According to Plato, however, nothing spatial or corporeal can be the cause of its own motion; and, since every such motion requires as cause a motion beyond itself, the ultimate cause of all corporeal motion must be an incorporeal self-motion, which he identifies as soul.[22] This is primary causation, whereas the motions of corporeal entities induced by other corporeal entities in motion and themselves inducing motion in still others can be only secondary causes.[23] Erratic or random motion must therefore, as motion, have its primary source in soul just as much as

[18] Since the spatial mirror is homogeneous and the ideas themselves are non-spatial, the reflections in space would not be locally distinct; and the Demiurge is conceived as delimiting them by geometrical configurations, thus representing spatially the "logical" distinctness of their non-spatial originals; *cf. Timaeus* 53 A8–B5.

[19] *Timaeus* 30 A3–6.

[20] *Timaeus* 46 E3–6, 47E–48A, 56 C3–7, 68E–69A; *cf.* Cherniss, 1944: 421–423 and 444.

[21] Vlastos (1939: 80–81): "Chaos . . . must, therefore, and for purely mechanical reasons, be in constant motion." Festugière (1946: 36, 40, 41) and most concisely in his later book (1949: 127): "la matière n'est pas seulement limite à l'Ordre, elle se trouve être par elle-même cause positive de désordre en ce qu'elle est mue, spontanément, de mouvements chaotiques." So Meldrum (1950: 66): "Noûs struggles to subdue ἀνάγκη.. The Demiurge does his best with these materials and succeeds on the whole, but to some extent they resist, and the evil of the world is simply this element of disorder that survives from chaos. So matter, *τὸ σωματοειδές*, is the *κακοποιόν*." In a note on this statement Meldrum adds (*loc. cit.*, n. 10): " 'Necessity' is a name for *τὸ σωματοειδές*, more precisely for the causal powers of matter, for the *αἰτίαι, ὅσαι μανωθεῖσαι φορήσεως τὸ τυχὸν ἄτακτον ἑκάστοτε ἐξεργάζονται* (Timaeus 46 E)." The term, *τὸ σωματοειδές* in this context is apparently a reminiscence of the myth of the *Politicus* (273 B4–C2), where in the absence of the Demiurge the increasing deviation of the cosmos from his instruction is charged to *τὸ σωματοειδὲς τῆς συγχράσεως* and it is said that *ὅσα χαλεπὰ καὶ ἄδικα ἐν οὐρανῷ γίγνεται* derive from the great disorder that prevailed before the Demiurge organized it into a cosmos. All interpretations such as the foregoing lean heavily upon this passage and upon the description of precosmical chaos in *Timaeus* 52 D–53 A, which they presume to be ultimate and unanalysable. What is referred to in this passage of the *Politicus* is undoubtedly the same as the errant cause of the *Timaeus* in operation; but the question is precisely whether erratic motion is an essential and irreducible characteristic of "corporeality" or is the resultant of "corporeality" and some other factor. In this connection it should be remembered, first, that "corporeality" itself is not for Plato an ultimate, unanalysable datum (*cf. Timaeus* 31 B) and, second, that in the *Politicus* not only are the precosmical disorder and the retrograde motion mythical, i.e. factors of the actual phenomenal world isolated for the purpose of description, but the retrograde motion during the course of which this disorder finally comes again to predominance is represented as a reaction of the tension created by the motion which the Demiurge imposes upon the universe (*Politicus* 270 A; see further note 44 *infra*).

A curious aspect of the interpretations that represent Platonic matter as thus self-moving is that they frequently represent it at the same time as "non-being" and, since it is the source of evil, represent evil therefore as somehow non-existent. So Festugière (1949: 129) says of what he calls Platonic Χώρα-matter: "Sous un premier aspect, elle apparaît comme une transposition physique de la notion dialectique de l'Autre: elle est un non-être relatif. . . . Sous un second aspect, étant mue spontanément de mouvements désordonnés . . , la *χώρα* matière apparaît comme un principe autonome de désordre. . . ." *Cf.* Greene's statements (1944: 305): "Ananke . . . is the negative substratum of phenomena" and (*ibid.*: 297) ". . . evil is somehow mere nonexistence (*μὴ ὄν*), or better, is otherness." In fact, Plato never suggests that evil is nonexistence or "otherness"; and, far from calling space *μὴ ὄν* or *θάτερον* he says *ταὐτὸν αὐτὴν ἀεὶ προσρητέον* (*Timaeus* 50 B 6–7) and expressly refers to it as *ὂν ἀεί* (*Timaeus* 52 A 8). The notion that space is a "transposition" or manifestation of the idea of otherness or non-being is entirely without foundation. The existence of space is for Plato a necessary inference from the analysis of phenomenal process (*Timaeus* 49 A–51B), and in characterizing it as *μετ' ἀναισθήσεως ἁπτὸν λογισμῷ τινι νόθῳ, μόγις πιστόν* (*Timaeus* 52 B2) he shows that he did not consider it to be a dialectical inference from the nature of the ideas themselves. On Aristotle, *Physics* 192 A6–8, which is Zeller's "evidence" (1922: 726, n. 3 and 733) for asserting that Platonic matter is non-being, *cf.* Cherniss, 1944: 92–96.

[22] *Phaedrus* 245 C5–246 A2, *Laws* 894 B8–896 C4.

[23] *Laws* 895 B, 897 A; *Timaeus* 46 D5–E2; *Phaedrus* 245 C5–9.

orderly motion has; to suppose that Plato in the *Timaeus* meant to make it a characteristic of corporeality *per se* is to assume that he there temporarily forgot or abandoned a fundamental tenet which he not only emphatically maintained both before and afterwards but which in fact is implied in the *Timaeus* itself.[24]

Mindful of this and of Plato's explicit statements that soul is the principle of all motion and change in the phenomenal world,[25] some interpreters have concluded that the cause of the random, disorderly motions must be an irrational element in the soul that according to the *Timaeus* pervades the universe and moves the heavenly bodies,[26] while others have ascribed this disorder to an evil world-soul opposed to this world-soul of the *Timaeus* and posited, they believe, in the *Laws*.[27] The text of the *Timaeus*, however, excludes the possibility of an irrational element in the world-soul there described,[28] while the existence of an evil world-soul as its adversary and the cause of the chaotic motions is not even mentioned in the *Timaeus* and is certainly not envisaged in the text of the *Laws* to which its proponents appeal.[29]

Nevertheless, Plato does categorically declare that soul is the cause of all good and evil in the phenomenal world.[30] It is recognized that this follows from the doctrine that soul is the cause of all motion in this world; but the contention that this is irreconcilable with the account of random and chaotic motion in the *Timaeus* as well as the attempts to reconcile the two by ascribing the chaotic motions to an irrational element in the world-soul or to an evil world-soul opposed to it both overlook the fact that these random, disorderly motions are expressly classified as secondary causes, which as secondary are to be regarded as somehow dependent upon the primary causality of psychical motion.[31]

It must first be observed, however, that some evil has its *immediate* source in such psychical motion, in other words that there are souls in the universe which produce evil effects because they are themselves evil.[32] Soul is good or bad ac-

[24] In the *Timaeus* the definition of soul is omitted as is all reference to self-motion in the *psychogonia* because to dwell upon it would have deprived the creation-myth of all literary plausibility (*cf.* Cherniss, 1944: 428–431, 455). Despite this, however, 37 B5 is meant to be a reminder of the doctrine, for there soul is referred to (*pace* Cornford, 1937: 95, n. 2) by the phrase, "that which is moved by itself," as 37 C 3–5 plainly shows. Moreover, *Timaeus* 46 D–E in making all corporeal motion secondary to the primary causation of soul assumes the doctrine that soul is self-motion, as is clear from *Phaedrus* 245 C5-9 and *Laws* 895 B, 897 A, where this classification of primary and secondary causation is presented as a consequence of that doctrine; it is consequently impossible to eliminate this passage, as Owen (1953: 95) tries to do by suggesting that it "may well contain only the raw material" of the doctrine of soul as self-motion. This doctrine, moreover, whatever the relative chronology of the *Phaedrus* and the *Timaeus* may be, was certainly not a "new development" of Plato's thought when he wrote the *Phaedrus* (*cf.* Cherniss, 1944: 433–442); it is involved in the "final demonstration" of the *Phaedo*, and the concept of self-motion itself is at least as early as the *Charmides* (168 E–169 A), a dialogue in which it is explicitly asserted that soul is the source of all good and evil for the body (156 E).

[25] *Phaedrus* 245 C9, 246 B6–7 (*cf. Laws* 896 D10–E2); *Laws* 892 A5–7, 896 A5–B1.

[26] *Cf.* Robin, 1935: 228–229; Cornford, 1937: 176–177, 205–206, 209–210; Morrow, 1950: 162–163.

[27] *Cf.* Wilamowitz-Moellendorff, 1919: 320–321. Dodds (1947: 21), though rejecting the notion that there is in the *Laws* an evil soul in the sense of "the Devil," holds that evil soul there and Necessity or the Errant Cause in the *Timaeus* are "symbols" of the same thing, "irrationality, the element both in man and in the κόσμος which is incompletely mastered by a rational will."

[28] The motion of the world-soul *as a whole* is called "ceaseless and intelligent life" (*Timaeus* 36 E). The Circle of the Different, which is sometimes taken to symbolize irrational motions in the world-soul (e.g. by Cornford, 1937: 76 and 208) has a constitution identical with that of the Circle of the Same (*Timaeus* 35 B 2 ff.); and, since the intermediate Being, Sameness, and Difference that are blended to form the soul are equally present in all parts of it and are all necessary for its rational processes (*Timaeus* 35 A, 37 A2–C5), this constitutive Difference cannot be made the cause of irrational motions (as is done e.g. by Robin, 1935: 228) without attributing these motions to the rational soul quâ rational. *Cf.* Cherniss, 1944: 410, n. 339, and 446, n. 387; Meldrum, 1950: 67–68.

[29] *Laws* 896 D10–E6, according to which we cannot say that a single soul controls and inhabits all moving things and so must control the heavens too but must assume at least two, beneficent soul and soul capable of the contrary. What follows in 898 C and 899 B proves that this is meant to assert the existence not of two world-souls, one good and the other evil, but of two *kinds* or *aspects* of soul (*cf. Phaedrus* 246 B6–C6). The question at issue is which kind of soul (*πότερον ψυχῆς γένοις* [897 B7]) controls the circuit of the heavens and the heavenly bodies; and, as the *ἀρίστη ψυχή* of 897 C7 and 898 C4 means "the best *kind* of soul," so *ἡ κακή* of 897 D1 and *ἡ ἐναντία* of 898 C4–5 means "the bad kind." It is not even asserted here that there is a single good world-soul; on the contrary, in the conclusion that soul which is the cause of the heavenly circuits must be completely virtuous the question whether this kind of soul is itself one or more than one is expressly left open (898 C6–8, *cf.* 899 B5–6: *ψυχὴ ἢ ψυχαὶ πάντων τούτων αἰτίαι*). In 904 A–E and 906 A–C there is no mention of any good or evil world-soul either but only of good and bad kinds of soul (*cf.* 904 B2–3: *ὅσον ἀγαθὸν ψυχῆς . . . τὸ δὲ κακόν*) and a plurality of both kinds (*cf.* 904 E 5–7).

[30] *Laws* 896 D5–8; *cf. Charmides* 156 E 6–8.

[31] *Timaeus* 46 D5–E6; *cf. Laws* 894E–895B, 896A–D.

[32] E.g. *Republic* 353 E; *Laws* 904 B–E, 906 B. It is not disorder in human affairs alone that is caused by evil

cording to its knowledge or ignorance, for soul is self-motion the mode or direction of which is determined by its knowledge, exact or erroneous, of the ideas and their relations to one another and which sets phenomena in motion in accordance with this knowledge or ignorance.[33] Among these objects of knowledge there are, moreover, ideas of certain phenomenal evils. Not of all, for much of what we term evil is merely negative, a phenomenal deficiency or deviation from the positive idea imperfectly reflected or imitated; but many such terms have a positive content too and as such must refer to real entities among the ideas. Of these latter, however, many again, though ideas of phenomenal evils, are not themselves evil. Such, for example, are the ideas of diseases which have constitutions of their own like living organisms; these are as ideas no more evil than are the ideas of man, of wolf, or of lion, but the phenomenal manifestations of all of these may by mutual interference in this world be evils relatively to one another. Similarly such ideas as pleasure, pain, and desire are not as ideas evil either; but their phenomenal manifestations, though they can be good, are frequently evil relatively to the circumstances and degree of their manifestation. Besides these, however, there are positive vices; of these the logic of Plato's doctrine requires that there be ideas, and the existence of such ideas he always, in consistency with that logic, maintained. Yet even these are not of themselves causes of evil in the phenomenal world. They are manifested as evil here only by soul which in ignorance mistakes their true nature and their relation to the Good, just as desire, pleasure, and pain have evil manifestations in this world only when the mode and direction of psychical motion is determined by error concerning their nature.[34]

What the ultimate cause of such error is, why soul should ever lapse from complete and accurate knowledge of the ideas, to this question Plato can, of course, give no adequate answer. He can only clothe in mythical language the assumption that this is so [35] or argue that epistemological considerations necessitate and justify the assumption.[36] But this assumption granted, it follows that soul, moving in ignorance or forgetfulness of the true nature of the ideas and especially of the relation obtaining between any of them and the idea of good, must cause evil in whatever part of the phenomenal world it affects by its motion, for it will misarrange the reflections of reality and may in its error even come to regard as real and take for the patterns of its action these spatial reflections themselves instead of their originals.[37] So positive evil in the world, both absolute and relative, is produced by the misguided motion of evil souls.[38] These, since they move in ignorance of

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souls, as is sometimes asserted (*cf.* Festugière, 1949: 110–111, 130); the evils produced by soul ἄνοιᾳ συγγενομένη include merely physical change and disorder, just as *Timaeus* 48 A5 ff. and 57E–58C show that the effects of the errant cause extend throughout the whole physical universe.

[33] *Cf. Laws* 896 E8–897 B5, where B1–4 gives the reason for the good and the evil effects of soul (*cf.* 898 B5–8). *Phaedrus* 246 B6–C6 expresses the same notion in mythical language, the vision of the ideas being the nourishment of the soul's plumage (*cf.* 248 B5–C2).

[34] See for ideas of diseases: *Phaedo* 105 C and *Timaeus* 89 B–C; of desire: *Philebus* 34 E (*cf.* Aristotle, *Topics* 147 A5–11); of positive vices: *Euthyphro* 5 D, *Republic* 402 C and 476 A, *Theaetetus* 186 A8, *Sophist* 251 A, *Laws* 964 C. On ideas of evil *cf.* Cherniss, 1944: 266, n. 175 (on p. 267) and for the opinions of later Platonists *ibid.*: 277, n. 176. The list of passages cited above refutes of itself Chilcott's statement (1923: 28) that, while ideas of evil appear in dialogues of "the middle period," this theory is criticized in the *Parmenides* and *Sophist* and that evil in the *Philebus* and *Timaeus* "has a purely negative existence." *Parmenides* 130 C5–E4 far from being a criticism of such ideas emphasizes Plato's refusal to breach the logic of his theory by rejecting them, and the doctrine of the *Sophist* does not eliminate such ideas so long as they have *positive* meaning (*cf.* Cherniss, 1944; 265–266; Ross, 1951: 169). Like many others, Chilcott (*ibid.*: 28 and 29) feels that the existence of such ideas is incompatible with *Republic* 509 B6–8, which he takes to mean that αὐτὸ τὸ ἀγαθόν is "the source of all existence." In fact, however, that passage need mean no more than that the ideas are what they are and are rightly known as such only in the light of the idea of good. This would be hard to reconcile with the existence of the ideas of positive evils that Plato clearly does posit only if by it he had meant, as Chilcott and many others under Neo-Platonic influence assume, that all ideas are derived from the Good or are created by it; but this the passage does not say, and that it was not so meant is shown by the interpretative summary at 517 C3–5, where it is said not that the idea of good is the source or origin of the ideas but that in the intelligible world it provides truth and intelligence (ἀλήθειαν καὶ νοῦν). *Cf.* with this 508 D4–6: when the soul fixes itself upon that which ἀλήθεια τε καὶ τὸ ὂν illuminate, ἐνόησέν τε καὶ ἔγνω αὐτὸ καὶ νοῦν ἔχειν φαίνεται.

[35] As e.g. in *Phaedrus* 248 C5–8 and in *Timaeus* 41 D6–7, where the ingredients of the immortal part of the human soul are said to be the same as those of the world-soul "but no longer so pure."

[36] As in the *Meno* (85 B–86 B) where it is contended that the soul "recollects" knowledge the source of which could not have been sensible experience and which therefore it must have possessed outside of the sensible world and have "forgotten"; *cf. Phaedo* 72 E–73 A, 74 A–76 E.

[37] Hence the tendency of evil souls "to cleave to the corporeal": *cf. Phaedo* 81 B–D (n.b. 81 B 3–8).

[38] That this is true of relative evils in the second sense above, e.g. evil desire, is obvious, for they are all manifested

truth, do not intend as evil the evil that they cause;[39] but the motions that they induce directly in phenomena are nevertheless induced deliberately and consequently cannot be the random motions of the errant cause, for these are motions transmitted by an object which has itself been set in motion by something else and are distinguished as secondary from the primary causality of soul.[40]

These random motions of secondary causality must have their ultimate source, however, in the primary causality of psychical self-motion; they do, in fact, follow inevitably the operation of this primary causality, whether it be good or evil. Consider the case of fully intelligent soul, the mode and direction of whose motion are in accord with its complete and constant knowledge of the ideas—the case, in short, of god or gods, for perfectly virtuous soul, that is fully intelligent soul, is what Plato means by god.[41] Soul of this kind in organizing phenomena moves them with a purpose perfectly good and sets them in motion proper to this end; but phenomena thus intentionally set in motion, since they are moved in a plenum of phenomenal reflections, must by their motions displace other phenomena, which in turn displace still others in directions unrelated to the intention of soul in moving the first directly. These secondary motions, as Plato calls them, the motions of phenomena induced by the movement of other phenomena and necessarily moving others in turn, intelligent soul in its demiurgic action seeks to employ for its own good end by making them conform to the plan of organization.[42] This it does by inducing in these secondary motions an alteration of direction, thus persuading them to cooperate with its original purpose;[43] but in so doing the demiurgic action again indirectly sets up in other phenomena another series of motions unrelated to its intention, motions that are neither intelligent nor purposive but accidental, random, and erratic. This is the errant cause or necessity, which reason can at best organize "for the most part" because the very act of organizing it begets a random residue of motion. Hence there is disorder which is the necessary incidental result of the action of soul—even of perfectly good soul—in delimiting and ordering the confused and indeterminate spatial reflections of reality. Evil souls, in their ignorance mistaking such disorder for good, may augment it; but the general flux of phenomena is not attributable either to their purposive action or to any spontaneous motion inherent in corporeality. As the *Timaeus* explains, it is the complex of secondary motions produced incidentally by the perfectly rational world-soul as it induces directly the rational motion of rotation in the spherical plenum of spatial figures, themselves delimited by reason.[44]

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only in soul as modes of its motion (*cf. Laws* 897 A 1–4), the result of misjudging the true relation to the good of the ideas of desire, pleasure, pain, etc. It is true, however, of relative evils in the first sense also, e.g. diseases that are natural organic units. These are, in the first place, evil only in the mutual interference of their phenomenal manifestations, an interference caused by soul's maladministration of phenomena. Moreover, they are all living organisms, and so their phenomenal manifestations, being animate, are directly controlled by soul; and Plato goes so far as to suggest that the phenomenal manifestations of all animate beings lower than man is the direct result of successively greater degrees of depravity of soul: *cf. Phaedo* 81 E–82 A, *Timaeus* 91 D–92 C (*cf.* 42 C), *Laws* 903 D3–E1 and 904 B6–E3; *Republic* 620 A–D and *Phaedrus* 249 B imply the same notion.

[39] *Laws* 860 D1 and the passages listed by Shorey *ad loc.* (1933: 640); *cf.* More, 1921: 243–261.

[40] *Timaeus* 46 E1–2, *Laws* 897 A4–5. *Cf.* for the following account of the errant cause Cherniss, 1944: 446–450. Morrow (1950: 153–154) holds that by "Necessity" in the *Timaeus* Plato means the dependable natures at the disposal of the Demiurge and the regularity of the effects that they produce upon one another: "the world on which the creator sets to work is characterized by necessity in the sense that specific effects follow regularly from specific causes." If this were true, Plato would hardly have described the mythical chaos on which the Demiurge sets to work as κινούμενον πλημμελῶς καὶ ἀτάκτως (*Timaeus* 30 A4–5) and he certainly would not have called necessity in the sense in which he here uses it the πλανωμένη αἰτία (*Timaeus* 48 A6–7) or identified it with the secondary causes which τὸ τυχὸν ἄτακτον ἑκάστοτε ἐξεργάζονται (*Timaeus* 46 E5–6). This last passage, as Meldrum says (1950: 66, n. 10 *ad fin.*), "forbids us to interpret Necessity in terms of Regularity of Sequence or natural law." The nature of "errant" motion is well exemplified by the statement in *Timaeus* 43 A7–B5: . . . βίᾳ δὲ ἐφέροντο καὶ ἤνεγκον, ὥστε τὸ μὲν ὅλον κινεῖσθαι ζῷον, ἀτάκτως μὴν ὅτῳ τύχοι προϊέναι καὶ ἀλόγως . . . καὶ πάντῃ κατὰ τοὺς ἐξ τόπους πλανώμενα προῆιεν.

[41] For proof of this *cf.* Cherniss, 1944: 602–610, especially 606–609. Apparently under the influence of Hackforth's article, which I have there criticized (*op. cit.*: 606–608), Greene (1944: 292, n. 95; *cf.* 287, n. 50; 311 and n. 235) goes so far as to write: "Plato does not say that either kind of soul, good or evil, is a god." This is, in fact, just what he does say in *Laws* 899 B5–8: "Since the causes of all these are soul or souls—and souls good in all virtue—, we shall say that they are gods. . . ." Virtuous soul is soul that has acquired intelligence (*Laws* 897 B 1–5 and 897 B8–C2). In the *Timaeus* the work of the Demiurge is the work of intelligence (*Timaeus* 47 E3–4, *cf. Laws* 966 E4), and the only entity that can have intelligence is soul (*Timaeus* 46 D5–6); and *Phaedrus* 249 C5–6 (*cf.* 247 D1–5) states that god's divinity is the result of constant contemplation of the ideas.

[42] *Timaeus* 68 E1–6, *Laws* 896 E8–897 B4 (*cf. Timaeus* 46 D1–E2).

[43] *Timaeus* 48 A2–5 and 56 C3–7.

[44] *Cf. Timaeus* 57 D–58 C; for the detailed interpretation

So, while all positive evil, both absolute and relative, in this world is caused directly by soul moving its objects intentionally but in ignorance of truth, soul, whether virtuous or evil, is also unintentionally and indirectly the source of evil that is necessarily incidental to its direct influence upon phenomena, whereas the negative evil inherent in the existence of the world as phenomenal is only the obverse of its goodness as a reflection—though only a *reflection* and therefore a derogation—of perfect reality. The question concerning the cause or source of evil is not the same, however, as the question of moral responsibility, with which it is often unconsciously confused. This confusion alone explains, I believe, such arguments as the one that it is grotesquely un-Platonic to make soul itself the cause of the instability of becoming and that therefore we must not take at its face value Plato's categorical assertion that soul is the cause of all motion and change.[45] For negative evil, whether the cause be considered the existence of space itself or the reflection that is an immediate consequence of its existence, nothing and no one is responsible; and for incidental evil, of which soul is certainly the ultimate cause though the necessary condition is the nature of phenomena once moved by soul to move one another, soul is not responsible either. There is moral responsibility only for the positive evil, of which soul is directly the cause, and for the ignorance of truth in which moving intentionally it produces such evil.

It may be said that this account fails to solve the "problem of evil," and so it does if evil is a problem to be solved only by demonstration of its nonexistence or by moral justification of it as the necessary condition for the existence of good.[46] Either of these "solutions" would have appeared to Plato to be an immoral falsification of the data of experience. Evil, like other phenomena, he regarded as something to be explained, not to be explained away; and all his remarks on the subject, when read in the light of this purpose, cohere to form a consistent account of evil which is a consequence of his analysis of the phenomenal world as a moving reflection in space of immutable, nonspatial reality.

of this passage and its implications see Cherniss, 1944: 444–445 and especially 448–450. In the myth of the *Politicus* the crucial passage, 273 B–D, implies the same explanation of the errant cause, there regarded in abstraction from intelligent causation. When the demiurgic guidance has been withdrawn, the world, left to itself, at first remembers the instruction of the Demiurge fairly accurately but with gradually increasing vagueness as forgetfulness increases and its old disorder gains dominance. The corporeality of its constitution, the demiurgic guidance now abstracted from it, is the cause of this. In other words, the direction given to phenomena by the motion of intelligence persists, though with diminishing effect, through the series of secondary causes for a time even when no further intelligent impulse is being given; but the further from the impulse of intelligence these series of secondary motions proceed the more the effects of that impulse wane and the greater in number become the merely random motions induced at each stage until at last the effect of the distant impulse of intelligence is virtually obliterated and the world is on the point of being nothing but the residual chaotic motions of secondary causality and of "running down" into "the limitless sea of dissimilarity."

[45] So Vlastos, 1939: 82. *Cf. Laws* 896 B1.

[46] *Cf.* Greene, 1944: 298.

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INTERNAL FINANCIAL REFORM OR EXTERNAL TAXATION: BRITAIN'S FISCAL CHOICE, 1763 *

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LIKE all arrangements by which wars are officially brought to a close, the Treaty of Paris of 1763, terminating the struggle familiarly known to Americans as the French and Indian War, presented the victor with new and demanding responsibilities. Thus the British, cast in the conqueror's role, found themselves faced with the problem of administering a vast, new territory wrested from their French and Spanish adversaries. Canada, the region west of the English settlements to the Mississippi, and the Floridas were the spoils of conquest. But to hold this princely domain, the British knew that they must effectively assert their authority over it.

The management of the ceded area was complicated by the presence of a large and hostile French population in Canada and by their equally bitter Indian allies domiciled in the Illinois country. The British, however, by tact and diplomacy succeeded in winning the French clergy and the French seigniors to their side. Through these two influential classes the reactions of the rest of the people were thereafter effectively controlled. The Indian presented a more formidable problem. The French had gained the good will and the loyalty of their dusky followers by respecting their humanity. On the whole they had accepted the natives as they found them and had made little effort, other than spiritually, to urge European customs upon them. Moreover, they had not migrated in such numbers as to alarm the aborigines or to disturb them in the enjoyment of their ancestral hunting grounds. With an abundance of gifts and supplies, the French had showered the red man with what he wanted and had bartered fairly with him for what he needed. The result had been the development of a relationship based upon the mutual respect of the two races.

The conquest of New France threatened to change all this. The easy-going French were replaced by the English, who made little effort to conceal their contempt for the Indian. The latter had already experienced the practical application of this attitude. They recalled with deep resentment and anger the many occasions upon which fur traders from the seaboard provinces had callously debauched them and cheated them during the course of subsequent exchanges. The prospect of accepting the overlordship of a race that indulged in such sharp practices was not pleasing to the tribesmen. To make matters worse, General Amherst, guided by a false sense of economy, chose the end of the war as the occasion for discontinuing the presents with which the English had attempted to woo the tribes during the fighting. It is not surprising that the latter were dissatisfied and listened to intriguing Frenchmen, who were bent upon inciting them against their erstwhile foes.

The gravity of the Indian problem was demonstrated by an uprising that burst upon the West early in 1763. Before it had run its course, attacks were launched against all the British posts west of the Allegheny Mountains. Only Detroit, Fort Pitt, and Fort Niagara escaped destruction. As the fury of the assault mounted, bands of Indians roamed the outlying settlements plundering, burning, killing, and scalping. In alarm, settlers abandoned their homes and rushed for the safety of more densely occupied areas. Temporarily, the savage attacks had turned back the tide of migration.

As we have already intimated, the British ministers were well aware of the need for military protection of the newly won territory. They recognized that the peace was little more than a truce in the protracted struggle with France. They were likewise sensible of the magnitude of the Indian problem created by the outcome of the war. Ac-

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cordingly, some weeks prior to the native revolt, a decision had been reached to support a protective force of ten thousand troops in the colonies.

At the time this judgment was made, it was generally conceded that the beneficiaries of the protection should assume a portion of the maintenance cost, estimated to be £350,000 a year. British subjects at home were already staggering under a heavy tax load, and there was every disposition to have their colonial compatriots share in the added burden. It fell to the lot of the Grenville ministry to carry this idea into effect. Consequently, George Grenville proposed two momentous measures, the Sugar Bill of 1764 and the Stamp Tax of 1765, both of which received Parliamentary approval. From these sources the imperial authorities expected to derive approximately £100,000 annually, or nearly one-third the cost of frontier defense.

Since the Sugar Act and the Stamp Tax excited so much colonial opposition, and since they set in motion the currents of action that ultimately led to separation from the Empire, the question naturally arises, could the levying of these and subsequent taxes have been avoided without endangering the imperial financial structure? An answer to this query is suggested in the following pages.

A consideration of the funded debt of England shows that country heavily weighted down by a burden which had been steadily increasing since its creation late in the seventeenth century. In 1755 it amounted to £71,755,000. By the end of the French and Indian War, it had jumped to £128,565,000.[1] There was likewise a growth over the war years of £2,800,000 in the unfunded debt.[2] The war had required an expenditure of £82,624,000, approximately three-fourths of which the government had been forced to borrow.[3] With the expansion of the debt, there had come a heavy additional interest charge. In 1755 the sum of £2,650,000 had been adequate for the purpose, but eight years later it had almost doubled.[4] These statistics gave the imperial authorities pause as they contemplated the expenditure necessary to maintain an army in the colonies. New sources of revenue were undeniably desirable. The easiest and most logical course to pursue, under the circumstances, was to turn to the American colonies for help. So concluded the king's ministers.

The justice of this move is scarcely open to legitimate challenge. Entirely apart from the debt structure at home, it was a reasonable expectation that the provincials should contribute to their own security. Grenville voiced such an opinion in 1765. Furthermore, there were indications that the English public shared this view. In 1767, William Samuel Johnson, London agent for Connecticut, reported widespread resentment among Englishmen stemming from the alleged favoritism shown the colonies in the allocation of financial burdens of this character. The impropriety of expecting the people at home to assume full financial responsibility in such matters was clear.

However reasonable it was to turn to the colonies for financial help, it did not prove to be a wise move. By this period the Americans had achieved a degree of economic and political maturity that raised grave doubts about their continued acquiescence in an empire relationship that permanently cast them in a subordinate role. One aspect of this growth was the hard-won right of the provincial assemblies to vote taxes and appropriate money. Would they, therefore, accept direct taxation by Parliament without a struggle? Similarly, evidence of this development was apparent in the pre-war and war-time trade of the colonies. Illicit trading with the French West Indies, that had developed in defiance of the Molasses Act of 1733, had not stopped with the outbreak of war, despite its obviously treasonable nature. It was evident that political loyalty was not strong enough to stand the practical test of economic gain. In view of these circumstances, surely with the return of peace there was little likelihood that the mercantile community would submit meekly to a policy of trade controls which it had openly flouted during the years of conflict.

Farseeing statesmen in charge of the finances of the country in 1763 might well have turned to internal fiscal reform rather than to external taxation. Such a policy would have eliminated the need for new taxes. It is also not inconceivable that such a move would have led to a lightening of the tax load in the homeland. This would have provided an excellent cushion for whatever public criticism may have developed of alleged favoritism shown the colonies in the financing of their own protection. Admittedly, however, such a plan would have been a difficult course to follow. It undoubtedly would have been opposed by the

[1] Great Britain, Parliament, House of Commons, *Accounts and papers: finance, finance accounts* **52**: 26–33, 1898.

[2] Great Britain, Parliament, House of Commons, *Accounts and papers: public income and expenditures* **35**: 302–305, 1868–1869.

[3] *Ibid.*, 708.

[4] *Ibid.*, 303–305.

placemen and practical politicians of that day. But it might have prevented the disruption of the Empire. In 1783, when the burden of the national debt had reached £230,000,000 and the annual interest charges amounted to £9,000,000,[5] a thorough overhauling of the government's finances was undertaken. By then it was too late. Had such reforms been initiated in 1763, perhaps the American Revolution would not have occurred.

It is impossible to obtain a complete and accurate description of British finances in the eighteenth century. In 1816 the House of Commons was informed that "owing to the confused state in which the Public Accounts were kept previous to 1800, no accurate view of the Income and Expenditures previous to that date could be made out."[6] It was not, in fact, until 1822 that balanced accounts were submitted to Parliament. Detailed information was available to that body, however, in the annual financial statements, which were published beginning in 1802.[7] Fortunately for the student of the period, Parliament created commissions at various times to make historical surveys of the financial operations of the government. One whose work began in 1780 made fifteen important reports. Another presented its conclusion in 1869, after twelve years of labor.[8] Consequently, although we lack a complete statement of the accounts, yet enough material is at hand to show the chaotic and costly methods which prevailed in the direction of the nation's finances.

One of the worst abuses of the times was the custom of delaying the auditing of the accounts of those to whom either the collection or the spending of national funds was entrusted. As late as 1783, the last complete statement of any paymaster general of the army to have passed through the office of the auditors was the joint report of Lord North and George Cooke for the year ending December 24, 1767.[9] The corresponding accounts for the treasurers of the navy were in no better condition. Those of the late George Grenville were the last to have been declared by 1783. They were for the year 1759.[10] The records reveal a most extraordinary situation. In 1783 the following summation of moneys issued had not been adjusted by the auditors: navy service, issues for twenty-one years, £74,000,000; army service, issues for eighteen years, £58,000,000; and issues to sub-accountants, approximately £39,000,000, a total of £171,000,000. And to this must be added the receipts for all provisions issued to the land forces in America and in the West Indies during the Revolutionary War.[11] None of these had been examined as yet by the royal accountants.

Eighteenth-century practice dictated that moneys raised by taxation be immediately appropriated for specific purposes. Accordingly, that which was to be disbursed to military personnel was deposited in the Bank of England to the account of officials charged with that function.[12] Normally these sums were far in excess of current needs. Hence, army paymasters general and naval treasurers ordinarily found themselves with large amounts of money in their custody for which there was no pressing demand. This was not frowned upon. On the contrary, custom permitted the officials to rent these inactive balances at interest until they were needed for governmental purposes.[13] This valuable perquisite of office was one of the conventional ways by which faithful political servants were rewarded.

Similarly, unexpended balances in the hands of officials at the close of their term of office were at their disposal until their accounts were adjusted.[14] Quite often audits were not performed during their lifetime. In such instances, the money was retained and used by their families or representatives pending a final settlement. The final account of Lord Holland, paymaster general of the army, was not submitted until seven years after his resignation. Charles Townshend, another incumbent, did not present his papers until eleven years subsequent to his retirement. The joint account of George Cooke and Thomas Townshend lay unexamined for a similar period, while that of Lord North and George Cooke gathered dust for twelve years.[15] The sums remaining in the hands of these individuals or their representatives as late as 1781, thirteen years after the last to hold the office

[5] *Ibid.*

[6] *Ibid.*, 326.

[7] *Ibid.*

[8] *Ibid.*, 327.

[9] Great Britain, Treasury, Commissioners of the Public Accounts, *Ninth report of the commissioners appointed to examine, take, and state the public accounts of the kingdom*, 27, 1783. Hereinafter these Reports will be cited according to their number as the *"Report of the commissioners of public accounts."*

[10] *Eighth report of the commissioners of public accounts*, 46, 1783.

[11] *Tenth report of the commissioners of public accounts*, 31 *et seq.*, 1783.

[12] *Fifth report of the commissioners of public accounts*, 18, 1781.

[13] *Ibid.*, 13, 18.

[14] *Ibid.*; *Tenth report of the commissioners of public accounts*, 18, 1783.

[15] *Fourth report of the commissioners of public accounts*, 6 *et seq.*, 1781.

had relinquished it, totaled £377,588.[16] If to this is added interest at the prevailing rate of four per cent over the period since they resigned, the figure expands to £672,423.[17] This represents the loss that the government was sustaining. Let us pursue statistics a bit further. All of these paymasters had been out of office at least since 1768. For the ensuing period, Richard Rigby held the post and had in his possession in excess of his needs an average of £450,166.[18] Adding this figure to the £672,423 already noted, the total is £1,122,589. It is instructive to note that had this amount been used to retire the public debt, which bore four per cent interest, there would have been a saving of £44,903, approximately the annual income produced by the Sugar Act.[19]

Conditions in the naval service varied little from those in the army. Treasurers of the navy found it difficult to balance individual ship books because of their inability to keep track of transferred seamen.[20] Accordingly, their business often continued long after they had given up their office. George Grenville's accounts were not completed until nine years after he had officially withdrawn from the service. The corresponding records of two other treasurers, Lord Barrington and Viscount Howe, required ten and eight years respectively.[21] This delay in clearing accounts was compounded by the apathy of the royal bookkeepers. Consequently, treasurers of the navy, like their army counterparts, the paymasters general, held public moneys for years after they had made their last disbursement. Grenville, Barrington, Howe, and a fourth, Sir Gilbert Elliot, had given up their seals as treasurers in 1762, 1765, 1770, and 1777 respectively. Yet the Commissioners of Public Accounts, appointed in 1780 to determine the state of the nation's finances, established claims against them in 1781 totaling £73,008.[22]

Under circumstances such as these, the public was not only deprived of the use of its own moneys but also frequently suffered losses of large sums. This was clearly indicated in the Commissioners' report of 1781. Here it was noted that there were delinquent accounts of more than sixty years' standing. The last return of Lord Lincoln, a paymaster general, was cleared to December 24, 1719. Between that date and June 25, 1720, he had received £473,127 from the Exchequer. The Commissioners could find no record of this money ever having been accounted for, nor could they find any evidence that it had been expended.[23] Similarly, the final statement of Anthony Viscount Falkland, treasurer of the navy, dated April 4, 1689, showed a balance of £27,611 due the government. In commenting upon these matters, the Commissioners exhibited a dry sense of humor. "A Debt," they noted, "that has subsisted for near a Century, may be presumed desperate."[24]

It should be noted in fairness to the central administration of the Empire that in an age of poor communications, the paymasters of the army and the treasurers of the navy were often confronted with legitimate and baffling delays in the settlement of accounts. This was especially true in instances where court proceedings were necessary before true balances could be presented. Naval accounting, for example, was often disrupted by long, drawn-out judicial action over prize vessels. Similarly, when local contractors were employed by the army in battle areas, resort to the courts was occasonally necessary before agreement could be reached on important items of supply. Such problems unquestionably postponed the final casting of accounts, but they fall short of a complete explanation of the situation. A system that encouraged governmental officials to use public moneys for private gain was not conducive to the development of an active interest in the rapid settlement of outstanding balances. And since custom decreed that this practice was commendable, relatively few there were who eschewed taking advantage of it. Under these circumstances, the efficient and orderly conduct of governmental finance was impossible. Waste and delinquency were the inevitable results.

The laxity characteristic of the collection of other public moneys was likewise a source of loss to the government. Receivers general of the land

[16] *Ibid.*, 27 *et seq.*

[17] *Ibid.*, 31 *et seq.*

[18] *Fifth report of the commissioners of public accounts*, 3, 14, 1781.

[19] This figure is an understatement of the situation. In the case of Lord Holland, for example, the Commissioners concluded in 1781 that he owed the government a balance of £256,456. By 1783, items not apparent at first had boosted this figure by an additional £90,795. See *Tenth report of the commissioners of public accounts*, 23 *et seq.*, 1783. See also, Beer, George L., *British colonial policy 1754–1765*, 284, New York, Macmillan, 1907.

[20] *Third report of the commissioners of public accounts*, 5, 1781.

[21] *Ibid.*, 6, 26.

[22] *Ibid.*, 26 *et seq.*

[23] *Fifth report of the commissioners of public accounts*, 17, 1781.

[24] *Third report of the commissioners of public accounts*, 3 *et seq.*, 1781.

tax were wont to retain their returns for long periods of time. This they did in defiance of a law requiring such funds to be lodged with the Exchequer within twenty days of their receipt. Their excuse was the inadequacy of their salaries and the difficulty of procuring remittances to London.[25] In 1780 the public accountants found that the average amount in the hands of the collectors was £334,061. They calculated that at the prevailing rate of interest, "the Public have been obliged to pay, for want of the Use of their Money," an annual charge of £13,362.[26]

Similar irregularities were found in the remission of the smaller miscellaneous taxes. Moneys were outstanding from the collection of first fruits of the clergy, tenths of the clergy, the sixpenny duty, and the one shilling duty, totaling £23,153.[27] The observation of the Commissioners on this subject embodied a complete statement of the kind of reform sorely needed. With a directness that must have made offenders wince, they declared that "there exists no Reason why the Public should not have the Custody and Use of the Public Money, rather than an Individual, until the Service to which it is appropriated, of whatever Nature that Service may be, calls for its Application: The Public Coffers are the safe Repository for Public Money." [28]

In the various governmental departments there were a number of sinecures that might well have been eliminated in the interest of economy. In the Exchequer there was an auditor of the receipt, a clerk of the pells, four tellers, four second clerks, two chamberlains, a tally cutter, and an usher. All but the second clerks and the tally cutter were life appointments empowering the incumbents to execute their duties by deputy. Traditionally the tally cutter enjoyed the same privilege, although he held office at the pleasure of the Lords of the Treasury.[29] Most of the business of the office, therefore, was conducted by representatives of the principal appointees. Horace Walpole must have had this in mind when he caustically characterized the auditor of the Exchequer as "the amplest sinecure in England, except the Archbishopric of Canterbury." [30] But among the lesser personnel there also was evidence of costly patronage. Depositions taken by the Commissioners in 1781 dramatically disclosed this situation. According to a deputy in one of the tellers' offices, not a single second clerk had made an appearance during the preceding thirty-six years; yet each one had enjoyed an annual income of more than £1,000.[31] Another testified that the same condition had prevailed in his office for twenty-six years.[32] In still another case, the current holder of one of these sinecures was found to be under age and, therefore, not legally eligible for remuneration. His father had been pocketing his fees.[33] Some of these offices, moreover, had long since outlived their usefulness. The tally cutter is a case in point. When reading was a rare art, the Exchequer kept its accounts with the public by means of tallies. The tally was a willow rod, roughly squared to an inch, and not over five feet long. Notches were carved on one side which varied in size according to the amount to be recorded. On each of the two sides adjoining the notched side, the amount was written. The stick was then split through the notches. One half was retained by the Treasury and the other was kept by the individual. This method was followed until 1782 when by act of Parliament written receipts were substituted for the tallies. Despite this action, however, the older practice persisted until 1836, when the office of chamberlain (where the tallies were cut) was abolished.[34] Other positions, such as that of usher of the Exchequer, continued to exist more as a matter of tradition than of need. The holder of this office was the beneficiary of the profits arising from supplying the Treasury and the Exchequer with stationery, "Turnery Ware," coal, and other articles. Fixed rates were established for many of these items; on others a profit of forty per cent was allowed. Repairs in both departments likewise fell within the jurisdiction of the office. On these a fee of three shillings six pence in the pound was collected.[35]

[25] *Fifth report of the commissioners of public accounts*, 5, 1781.
[26] *Ibid.*, 7.
[27] *Second report of the commissioners of public accounts*, 6 *et seq.*, 1781.
[28] *Ibid.*, 8.
[29] *Sixth report of the commissioners of public accounts*, 13, 143, 152, 1782.
[30] Walpole, Horace, *Memoirs of the reign of King George the Third* 1: 239, 4 v., London, R. Bentley, 1845.
[31] *Sixth report of the commissioners of public accounts*, 137 *et seq.*, 1782.
[32] *Ibid.*, 139.
[33] *Ibid.*, 137.
[34] *Ibid.*, 147; Great Britain, Parliament, House of Commons, *Accounts and papers: public income and expenditures* 35: 336 *et seq.*
[35] *Sixth report of the commissioners of public accounts*, 26 *et seq.*, 1782.

In their survey, the Commissioners of Accounts were concerned primarily with the most glaring faults of administration. It is obvious, however, that the system as a whole was conducted largely on the patronage level. Characteristic was the action of Charles Townshend, paymaster general of the army in 1765, in appointing as a ledger keeper a man whose eyesight was so impaired he could not perform his duties. In 1783 he was still on the payroll, his responsibilities being discharged by a deputy, who likewise shared in the prevailing largess.[36]

The pension system was another source of drain upon the country's finances. These annuities were granted largely as a reward for political, military, and naval services rendered. Not infrequently, moreover, they were awarded as charitable gifts. Actual amounts distributed for these purposes cannot be accurately determined. In 1760, over £100,000 were marked for pensions. In this category, also, was another £50,000 available for "Secret Service" and £138,000 for "Contingencies of Divers Nature." [37] The advantage of this system to the throne lay in the fact that the anticipation of a pension generally exacted compliance and subserviency to the royal will. When this was not forthcoming, no subsidy was granted. Thus in 1775 when Lord North suggested to the king that the pension of the ailing Lord Chatham be enlarged as a means of winning the allegiance of the statesman and his family, George III rejected the proposal. In a scorching reference to Chatham and his family, he said, "As to any gratitude to be expected from him or his family, that would be absurd when the whole tenour of their lives have shown them void of that most honourable sentiment." [38] So much a part of the conduct of government was this policy of royal rewards that Edmund Burke, in a plea for reform before the House of Commons in February, 1780, did not urge its abolition but only the limitation of the available amount to £60,000. This would, however, have meant a substantial saving.[39]

There can be no doubt but that even a mild reform of the imperial fiscal system would have provided the home country with more than the £100,000 it expected to derive from taxing the colonies. It seems equally clear that England in 1763 claimed no statesman willing and capable enough to push such a reform to fruition. The beneficiaries of the system, actual and prospective, were not interested in legislating themselves and their supporters out of profitable livings. They believed that they had a legal and moral right to the proceeds of office. Disturbance of these vested rights ran counter to the political philosophy of the day. A Robert Walpole had provided well for his family. His second son, Sir Edward, held the office of clerk of the pells for life.[40] His third son, Horace, drew enough from his lifetime appointment as usher of the Exchequer to devote his days to political and literary pursuits.[41] Although profiting from the system, Horace was aware of its evils. In a letter to a friend in which he prophesied the fall of Europe and the rise of America, he added, "I entertain myself with the idea of a future senate in Carolina and Virginia, where their future patriots will harangue on the austere and incorruptible virtue of the ancient English! will tell their auditors of our disinterestedness and scorn of bribes and pensions, and make us blush in our graves at their ridiculous panegyrics." [42] Others, perhaps less introspective than Horace Walpole, accepted and dispensed patronage wholly untroubled by its implications. Such a one was George Grenville, treasurer of the navy from 1756 to 1762. One of his sons, Earl Temple, secured a tellership for life; [43] another, a second clerkship in the same office.[44] Obviously Henry Fox (later Lord Holland), Charles Townshend, and Lord North, as paymasters general of the army, fared well at the hands of the current financial customs. Fox is credited with amassing a princely fortune from the interest upon outstanding balances in his hands. With it he was able to meet the gambling debts of his brilliant sons.[45]

[36] *Ibid.*, 10, 64.

[37] Namier, L. B., *The structure of politics at the accession of George III*, 1: table facing page 230, 2 v., London, Macmillan, 1929.

[38] Lord North to the King, August 8, 1775, Fortesque, Sir John, ed., *The correspondence of King George the Third from 1760 to December 1783*, 3: 240, 6 v., London, Macmillan, 1927–1928; The King to Lord North, August 9, 1775, *ibid.*, 242.

[39] Burke, Edmund, *The works of the Right Honorable Edmund Burke* 2: 328, 4 v., Boston, Little, Brown, and Company, 1869.

[40] *Sixth report of the commissioners of public accounts*, 17, 118, 1782.

[41] *Ibid.*, 26, 161.

[42] Horace Walpole to Sir Horace Mann, May 6, 1770, Mumby, F. A., *George III and the American Revolution*, 290 *et seq.*, New York, Houghton Mifflin, 1923.

[43] *Sixth report of the commissioners of public accounts*, 143, 1782.

[44] *Ibid.*

[45] Morley, John, *Burke*, 134, London, Macmillan, 1907; Trevelyan, George O., *The early history of Charles James Fox*, 22 *et seq.*, New York, Harper and Brothers, 1899.

The Duke of Newcastle, given a lifetime appointment in 1751 as an auditor in the Exchequer, and the Earl of Northington, a similar grant in 1766 as a teller in the same office, likewise shared in the government's bounty.[46] As Burke stated, "Very few men of great families and extensive connections but will feel the smart of a cutting reform, in some close relation, some bosom friend, some pleasant acquaintance, some dear, protected dependant." [47] The opposition to change which these men and their kind could muster was formidable.

The extent of the waste in the British treasury was not publicly known until 1780. That year, at the insistence of Edmund Burke, commissioners were appointed to investigate the situation.[48] Their reports led to improvements in the nation's finances. Not until well into the nineteenth century, however, was much accomplished. The restrained pace at which reform was effected was foreseen by the Commissioners. Tucked away in their voluminous report of 1781 is a paragraph providing us with not only the current but also an ageless explanation of this phenomenon. "We are well aware," they said, "of the Difficulties that must forever attend the introducing Novelty of Form into ancient Offices, framed by the Wisdom of our Ancestors, and established by the Experience of Ages; they are considered as incapable of Improvement; the Officers educated in, and accustomed to the Forms in Use, are insensible of their Defects, or, if they feel them, have no Leisure, often no Ability, seldom any Inclination, to correct them; alarmed at the Idea of Innovation, they resist the Proposal of a Regulation, because it is a Change, though from a perplexed and intricate, to a more simple and intelligible System." [49]

It would be idle to state categorically that had financial reforms been undertaken twenty years earlier the American Revolution would not have occurred. There were, of course, many other influences at work pushing inexorably toward some major readjustment between parent land and colonial offspring. The thought will not down, however, that such a readjustment need not have been a violent one marked by complete repudiation of the one group within the family by the other. The effort to raise money in the colonies by taxation after 1763 certainly was a major factor in stimulating that kind of reaction among the provincials. It focused attention upon long-standing commercial differences with the home country and it was associated with the conflicting British and colonial interests in the new West. It raised constitutional questions and it laid the basis for charges of imperial tyranny. In short, it served as a powerful catalytic agency. What would have been the course of history had this ferment not begun? The possibility of the formulation of policies for the accommodation of imperial-colonial interests would have been infinitely better had not the pressure for raising money in the provinces been present. Was the *Annual Register* right in 1780 when it reported the idea that "had such savings [governmental savings] taken place in time, America would still have been a part of our strength and glory"? [50]

[46] *Sixth report of the commissioners of public accounts*, 35, 143, 1782.

[47] Burke, Edmund, *The works of the Right Honorable Edmund Burke* 2: 179.

[48] Morley, John, *Burke*, 137, 140; *Annual Register, or a view of the history, politics, and literature, for the year 1780*, 114–186, London, printed for J. Dodsley, in Pall-mall, 1781.

[49] *Fifth report of the commissioners of public accounts*, 19, 1781.

[50] *Annual Register, 1780*, 118.

SOME DEMOGRAPHIC ASPECTS OF AGING

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VIEWED as a whole the "problem of aging" is no problem at all. It is only the pessimistic way of looking at a great triumph of civilization. The population of the United States, and of the Western world in general, has grown older because it has learned to be efficient in the renewal of its life. Today one thousand births yield about twice as large a fund of life as they produced in Colonial times, and as they produce today among more than one-half of the world's population. We are a relatively old population partly because so many of us have been successful in achieving our goal of individual survival. So, with a perversity that is strictly human, we insist on considering the aggregate result of our individual successes as a "problem."

It is true that much more than the attainment of low death dates is involved in the aging process. Indeed, as we shall see presently, movements of the birth rate are likely to be more important in determining the speed of the aging process than changes in the death rate. The two rates, however, are closely connected. It is at least possible to doubt that death rates would have fallen so low if birth rates had failed to decline. Moreover, it is difficult to imagine that we could have achieved anything like the existing level of material prosperity if modern mortality had been coupled with pre-modern fertility. Such a combination would yield increases of at least 3 per cent per year. This rate of increase doubles a population in twenty-three years, and yields a twentyfold increase in a century. If our population had grown at that rate after 1850, as it did in fact for a century before, we would now have more than 450 million, and by the year 2000 we would have more than 2 billion people in the United States. In the presence of such growth we may doubt the possibility of attaining either prosperity or exceptional health.

To suggest that the aging of the population should be viewed as an asset is, of course, not at all to suggest that there are no problems of economic and social adaptation to be faced. Naturally there are such problems, for the changes in the composition of the population have been rapid and have brought with them changes in the labor force and in the burden of dependency.

Figure 1 shows the trend by decades from 1900 to 1950 for the population in four major groups: under age 20, 20–44, 45–64, and 65 or more years of age. No special virtue except that of convenience is claimed for this grouping. The group under 20 years may be taken to stand approximately for young dependents although many persons begin to support themselves in their 'teens. We may take 20–44 as the ages from which the younger labor force is drawn, and 45–64 as those furnishing the older labor force. Old age is also arbitrarily defined as above age 65 without any imputation that all persons over that age are unproductive, or that age 65 ought to be the age of retirement.

The left hand panel shows that the largest increases from 1900 to 1950 came in the young working ages, and the next largest in the older working ages. Additions to the working ages were very much larger than additions to the ages of dependency. In spite of the sharp reduction in births in the depression decade, the population under age 20 increased by 17 million after 1900, whereas the population over age 65 grew only 9 million. The right hand panel of figure 1 presents the same data on a logarithmic scale to permit a comparison of rates of increase. It is evident that the increase of the aged, although the smallest in absolute size, was much the most rapid. Indeed, between 1900 and 1950, while the total population was about doubling, that over age 65 increased fourfold. It is also evident from the chart that the older workers were increasing at a faster rate than either the younger workers or the population under age 20.

To gain some impression of future trends it is necessary to speculate about the future course of mortality. First of all, it is evident that nothing

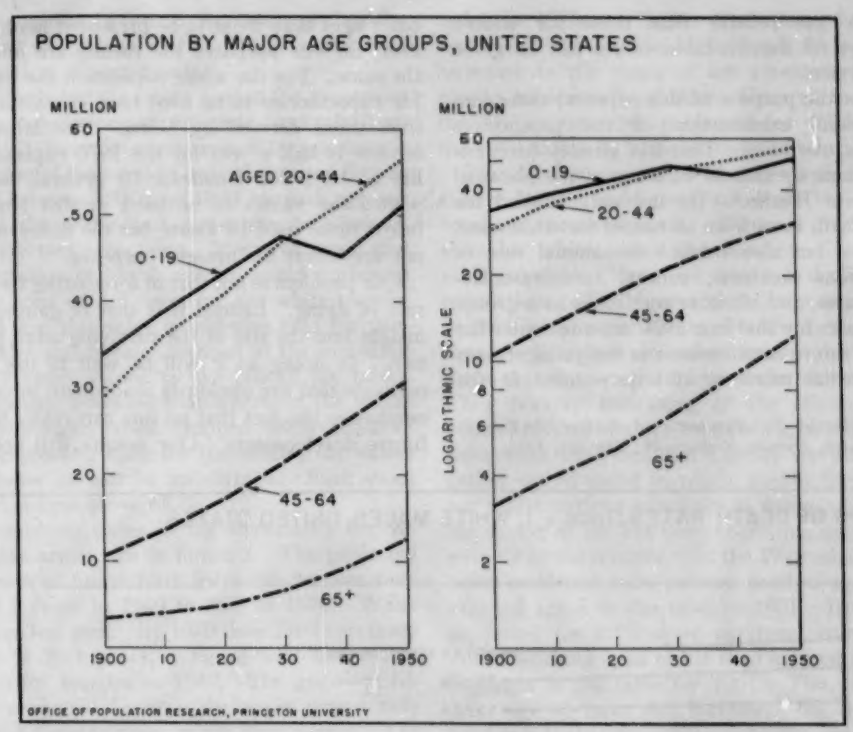

FIG. 1.

useful can be said in this context if there is a full-scale global war, except that the "problem of the aged" may be quickly, if unpleasantly, resolved. Apart from this contingency the task of predicting death rates is difficult enough. In the present case there is one saving factor. The purpose of the analysis is to direct attention to the expected rapid increase of the older segments of the population. It will be well, therefore, not to exaggerate this increase; hence to project a conservative future decline in mortality. This bias is a useful one, for most forecasts have been outstripped by the actual course of events.

Figure 2, which relates to white males, summarizes the recent trends of death rates for the various age groups. In it the life-table death rates for the three-year period centered on 1930 are connected by straight lines with the corresponding values for 1949, the rates being plotted on a logarithmic scale. It is evident that the intermediate points fall rather close to the lines connecting the values for 1930 and 1949. In other words, during the nineteen years from 1930 to 1949 the death rates for each age group tended to decline at rather constant rates. The decline was most rapid in the youngest ages. It was 3.4 per cent per year for infants, 6.7 per cent between 1 and 5 years, and remained above 3 per cent until age 40. By age 50 the decline had dwindled to less than 1 per cent, and by age 60, to half of 1 per cent. In other words the spectacular reductions in the risk of death took place at ages under 50, and after that age the declines were rather gradual.

The data for white females present much the same picture, except that the death rates were substantially lower and declined more rapidly than those for males. Annual rates of decline exceeded 1 per cent up to age 80. Again straight lines connecting the logarithms of the rates for 1930 and 1949 describe the trend fairly well.

The data for non-whites tell much the same story, except that the death rates were uniformly higher than the corresponding ones for whites, and above age 15 they were falling much more rapidly. Unfortunately the data for non-whites

are much less reliable than those for whites. Above age 65 the life-table values can be given little credence.

It is not the purpose of this paper to enter into a meticulous examination of the prospective changes in mortality. That has already been excellently done by Harold F. Dorn of the National Institutes of Health.[1] He not only studied the trend of death rates from all causes for whites and non-whites, but also studied the annual rate of change from accidents, cancer, cardiovascular-renal diseases, and all other conditions as a group. His forecasts for the year 1970 assume somewhat less rapid future improvement in the younger ages and somewhat more rapid improvement in the older ages than those to be presented here. However, for our purposes the results are effectively the same. For the white population the projected life expectancies to be used here run a bit higher than those derived by Dorn. The largest difference is half a year for the 1970 expectation of life at birth for females. In general, for non-whites the values to be used in this study run below those used by Dorn, but the differences are not important for present purposes.

Our problem is not that of forecasting the future rate of dying. Instead it is that of gaining some insight into the size of the surviving adult population. In doing so it will be well to use simple methods that are obviously inadequate in order to emphasize the fact that no one can really forecast future developments. Our results will not show

[1] Dorn, Harold F., Prospects of further decline in mortality rates, *Human Biology* 24: 234-261, 1952.

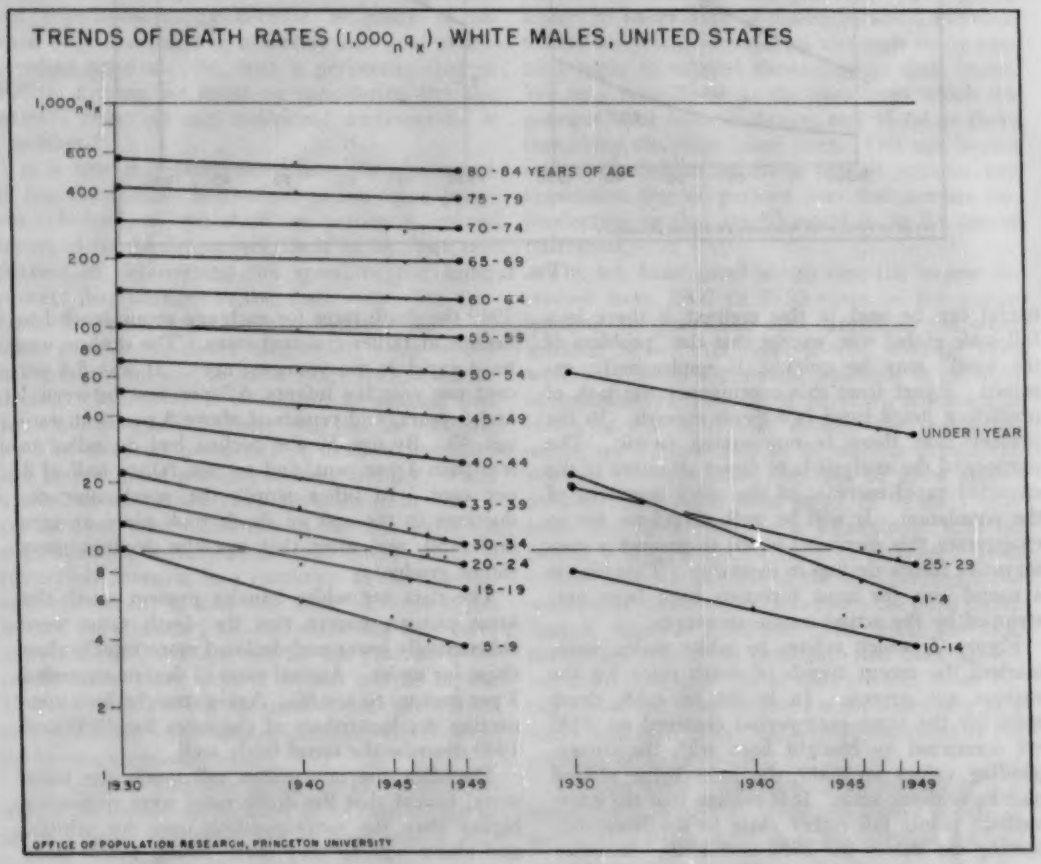

FIG. 2.

the precise nature of future developments, but the general sort of situation that may be expected from a continuation of past trends.

Accordingly the life tables needed for projecting surviving populations at five-year intervals from 1950 to 1970 have been constructed by assuming a continuation of the average annual per cent decline observed between 1930 and 1949 for each age-sex group of the white and non-white populations. There were two exceptions. It was assumed that the expectation of life at age 85 would remain at the 1949 level of 3.7 years for the white population, and that this value would also hold for non-white males and females, instead of the incredible 8 and 11 years shown by the official life tables. It was further assumed that wherever the values of the non-white tables dropped below those of the corresponding table for the whites, the values of the latter should be substituted. Such cases occurred at ages above 65.

The resulting gains in life expectancy for selected ages are shown in figure 3. The projected expectation of life at birth for white females rises from 71.5 years in 1949 to 76.9 in 1970. White males fare less well. By 1970 their life expectancy at birth is 70.1 years, a figure well below that observed for females in 1949. The growing difference projected between the sexes means only that the difference grew in the base period. The projected values for non-white males at birth in 1970 are well above those for white males in 1949, and the projected values for non-white females in 1970 are higher than those for white males in 1970. At the higher ages the projected increases in the years of life remaining are, of course, substantially smaller. Even so, for age 60 the life expectancies projected for 1970 exceed the 1949 values by between one and two years.

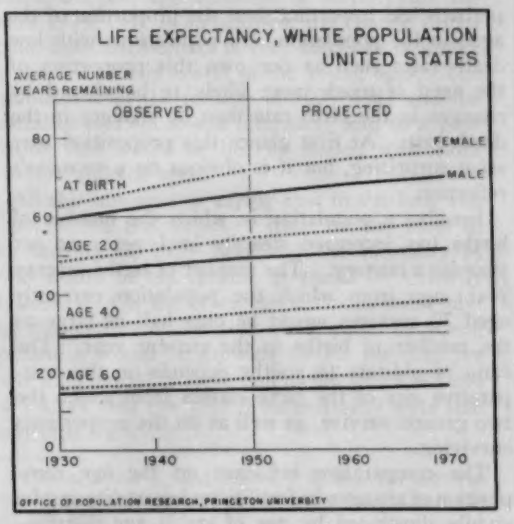

FIG. 3.

The significance of these changes is shown more clearly by the number of survivors to each age from one thousand births. Figure 4 gives the results for white males and females. It must be understood that here we are not dealing with populations. The figures show only what would be the life history of a class of births if, as it passed through life, it were submitted to the risks of death that characterized a particular calendar year. They portray something of the efficiency with which life is maintained by showing, as areas under the curves, the years that a group starting life together would spend in youth, middle life, and old age under various schedules of dying. Obviously the saving of life has been enormous since the beginning of the century. In the 1949 table for both sexes combined more persons reached age 55 than reached age 5 in the table of 1901. In the table projected for 1970 more survivors complete the entire working span of life than entered the working ages in the table for 1901. The years lived after age 65 have also increased, but the significant fact is that the additions to the working ages have been very much larger than those to the ages under 20 and over 65 taken together. Moreover, the life table projected for 1970 implies larger additions between ages 20 and 65 than before and after those ages.

The years gained in the working ages through the improvement in mortality have exceeded those added in the dependent ages but, as figure 5 shows, the ratio of years in dependency to years in the working ages has risen for females and may well continue to rise. For males the ratio has remained almost steady since 1930 and very little rise is projected. Of course this only means that the rise in the ratio of years in old age to years in the working span has been almost cancelled by the fall in the ratio of years spent in youth to years in the working ages. On the strength of such figures one can only conclude that the great economies that result from our growing ability to survive through the working span of life have not been, and in the near future are not likely to be, cancelled by the growing burden of years spent in old age.

Thus far we have dealt with only one part of

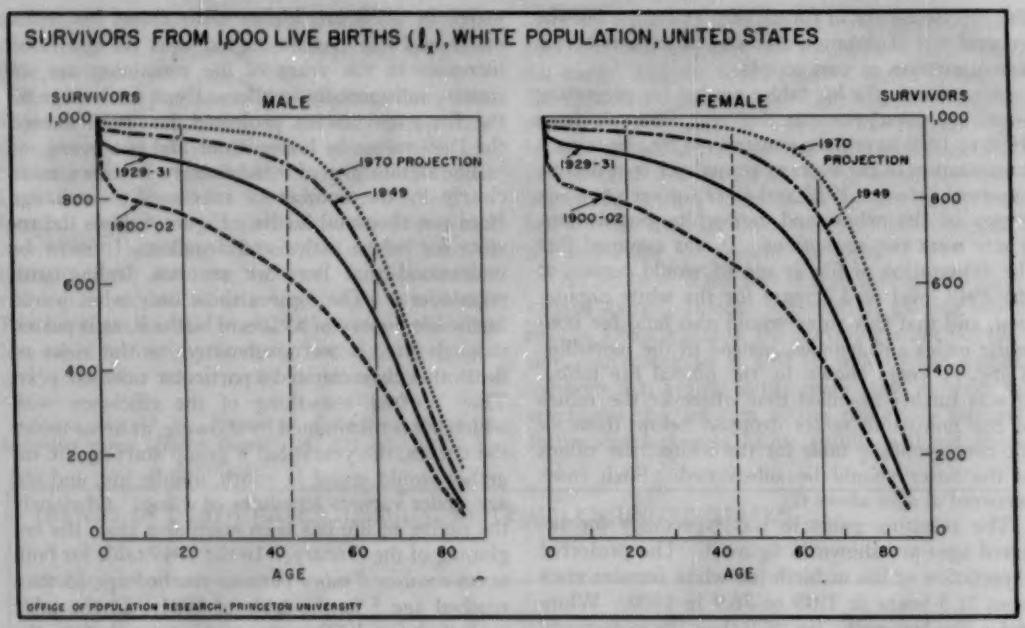

FIG. 4.

the problem. We have considered only the implications of changes in mortality as they are reflected in the life patterns of a group of people moving through the life span. But these are not populations. The number of the aged during the next sixty-five years will depend, apart from migration, on death rates during the next sixty-five years. So far as their social-economic impact is concerned, however, the number of the aged is perhaps less important than the proportion of the aged in the population. In populations with low death rates such as our own this proportion of the aged is much more likely to be altered by changes in the birth rate than by changes in the death rate. At first glance this proposition may seem surprising, but it is obvious on a moment's reflection.

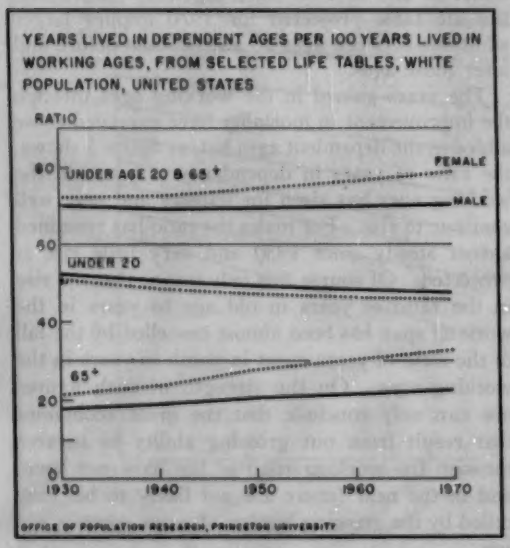

FIG. 5.

Imagine a population in which the number of births has increased steadily at 1 per cent per year for a century. The number of births seventy years ago from which the population currently aged 70 survives would be only half as large as the number of births in the current year. The ratio of oldsters to youths depends on the comparative size of the birth classes from which the two groups survive, as well as on the proportions surviving.

The comparative influence on the age composition of changes in fertility and mortality can be vividly illustrated by use of stable age distribu-

tions. In 1925 Dublin and Lotka showed that closed populations with fixed age schedules of fertility and mortality would approach characteristic age distributions as a limit.[2] These they called the stable age distributions. Figure 6 permits the comparison of several stable age distributions for the white population of the United States with the actual white population of 1950. The actual distribution shows the large number under age 10 contributed by the high birth rates of the past decade, the sharp drop in the early adult years from the reduced number of births in the depression decade, and the heavy loading of the ages from 25–44 from the large birth classes prior to 1925. It also shows rather low proportions of the aged who represent survivors of the small birth classes of years prior to 1885, when the population was only a little more than a third as large as at present.

By contrast both of the lines that are very high in the young ages show the age composition that would ultimately arise if there were no migration and if the rates at which women in the various ages bore children during the year 1948 were to be indefinitely maintained. The higher of the two lines in the young ages assumes also a continuance of the rates of dying given by the 1948 life table, and the lower assumes the rates of dying projected for the 1970 life table. Obviously the projected change in mortality between 1948 and 1970 makes an almost trivial difference.

When, however, the mortality of 1948 is retained, but the age schedule of fertility for the years 1939–1941 is substituted, the change in the stable age distributions is very large, as is shown by the line that is lowest in the young ages. The combination of the fertility of 1939–1941 and the mortality of 1948 would ultimately lead to a population that would be heavily loaded with older workers and the aged.

All this amounts to saying that in the long run a population that renewed itself on the basis of the fertility of 1939–1941 and the mortality of 1948 would be old because it would be virtually stationary. On the other hand, a population having the fertility of the year 1948 would be young because it would be growing by 1.3 per cent per year if its mortality were that of the year 1948 and by 1.4 per cent per year if its mortality were that projected for 1970.

[2] Dublin, Louis I., and Alfred J. Lotka, On the true rate of natural increase, *Jour. Amer. Statis. Assoc.* **20**: 305–339, 1925.

Lest those who think it advantageous to maintain a young population become too enthusiastic about the virtues of a high birth rate, we should hasten to point out that youth is to be maintained in spite of low death rates only at the cost of perpetual growth. The rate of growth that would be generated by a maintenance of 1948 fertility and mortality is a very high one. Indeed a continuation of a 1.3 per cent rate of increase from 1950 onward would give the United States a population of half a billion within the lifetime of persons already born. It may be doubted that the advantages of youth would cancel the liabilities of maintaining such a continuous rate of increase. Much of our youth in the past has been a function of our growing number of births rather than of our unfavorable mortality.

Thus far we have considered some of the implications of changes in mortality and fertility for the age composition of the population. In the immediate future the problem is different. The labor force and the aged dependents of the years up to 1970 are already born. Births in the interval between now and 1970 are chiefly relevant in relation to the load of young dependents that is to be coupled with that of the aged for the people of working age to support.

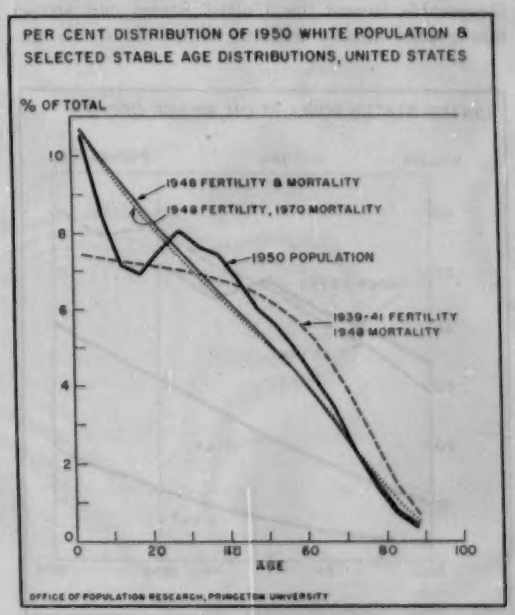

FIG. 6.

It would carry us too far afield to give careful consideration to the probable trend of births during the next two decades, and in any case our conclusions would be highly tentative. Perhaps it is sufficient to observe that the number of births may well decline in the next five years. Two principal factors support this expectation. In the first place, the short birth classes of the depression decade are now entering the childbearing ages, so that the number of potential parents is being reduced. In the second place, a considerable part of the recent increase in births has come from a progressive decline in the age at marriage. The number of births should fall off when this decline stops. If an economic recession were to lift the age at marriage, the ensuing decline in the number of births might be quite sharp. Any such decline, however, should not last very long for by the early 1960's the large birth classes of the 1940's will enter the childbearing ages and increase the number of potential parents very rapidly. By 1970 there is a strong probability that the population under age 20 will be considerably larger than it is now. Over the next two decades it is rather unlikely that the increase of the aged will be counterbalanced by any decline in young dependents.

The age structure of the population could also be altered rather quickly by migration, for in the foreseeable future the United States can attract immigrants almost without limit. It seems unlikely, however, that we shall be willing to accept immigration on a scale that will change the fundamentals of our position in the next two or three decades.

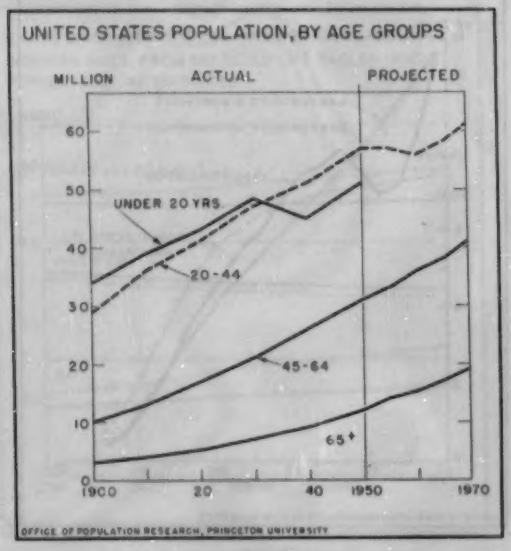

FIG. 7.

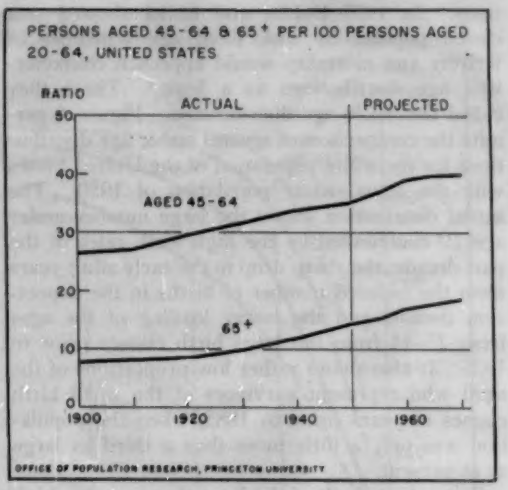

FIG. 8.

Turning then to speculate about the trend of the adult population up to 1970, we may neglect migration and use the somewhat rough and ready procedure of also neglecting both the underenumeration of children and the number of troops overseas in 1950. Such obvious omissions have the advantage of not obscuring the fundamentals of the situation while emphasizing the fact that no precise forecast is intended. Figure 7 gives the total population over age 20 by broad age groups at five-year intervals between 1950 and 1970. It is obtained by using the projected life tables already discussed to move the 1950 census count forward in age and time by five-year steps to 1970. If there is no major migration, and no global war, and if death rates decline during the two decades after 1950 at the same rates they declined during the two decades before 1950, figure 7 shows approximately what will happen.

On these projections the increase in the number of people of working age is about 4 million less between 1950 and 1970 than it was between 1930 and 1950. In the older working ages the population increases about one million more between 1950 and 1970 than it did between 1930 and 1950. It is in the young working ages that growth is most severely cut. Between 1930 and 1950 it increased by 9.7 million, whereas the increase pro-

jected for 1950 to 1970 is only 4.7 million. The increase projected for the two decades following 1950 is less than half that which occurred in the two decades prior to 1950. On these assumptions we must expect smaller increases in the number of people of working ages in the near future than in the recent past, and a progressive aging of the labor force.

Meanwhile the increase of the aged during the twenty years following 1950 should be about one million larger than it was during the twenty years before 1950. The estimate is probably conservative because it makes no allowance for possible acceleration in the decline of death rates up to 85 and no allowance at all for improvement after that age. There is every prospect that the aged sector of the population will grow the most rapidly. The projected figure of nearly 19 million persons over age 65 in 1970 is almost three times the number in 1930. That for the population in the older working ages is nearly twice the number in 1930, and that for the population in the younger working ages in 1970 is only about one-third higher than the number in 1930.

As may be seen in figure 8, the proportion of persons in the working ages who are over 45 years rose from 31 per cent in 1930 to 35 per cent in 1950 and is projected to rise to 40 per cent in 1970. Meanwhile the ratio of the aged to the people in the working ages rose from 10 per cent in 1930 to 14 per cent in 1950 and is projected to rise to 18 per cent in 1970.

The trends toward an aging labor force and toward higher ratios of persons in the dependent ages to persons in the working ages are fairly rapid and the magnitudes are of some consequence. Just to keep matters in perspective, however, we should note that from the point of view of economic fundamentals we are only moving from an exceptionally favorable situation toward one that is very favorable indeed as such matters go. Most of the world's population probably suffers the terrible cost of losing before age 20 more than 40 per cent of those born. Any population such as our own that does not lose 40 per cent of those born until after age 65 is making highly efficient use of its human resources. So long as its births are sufficient to maintain the population and are not so numerous as to create a damaging growth, our problems do not lie in the fundamentals of the demographic situation. Instead they are the simpler ones of social-economic adjustment to the fact that we have learned how to avoid the gross waste of premature death.

INDUSTRIAL TRENDS AFFECTING THE OLDER WORKER

L. S. BARRUS

Assistant to the President, The Cleveland Twist Drill Company

(Read April 23, 1953, in the Symposium on Social and Economic Problems of Aging)

I. THE ISSUE OF FLEXIBLE OR AUTOMATIC RETIREMENT

IN THE YEAR 1880, a twelve-year-old, named Richard, strode into a small machine shop and asked for a job. He got it—on condition he went to school as well. The small boy made good, became in time a foreman, and later his company's ace salesman and service engineer. After some fifty-five years of service he retired for a well earned rest. It lasted just six months! Back he came to work. A decade later, the production battle of World War II was at its peak, and working ten hours a day, six days a week Richard was doing his important bit as a defense plant trouble shooter. Since then another decade has flown, in his seventy-third year of service Richard is slowing down a little—his vacations are longer—but he is still first on deck at 7:30 sharp, still dancing a jig for the girls in the office elevator, still helping customers solve their tool problems.

Richard is not in sympathy with automatic retirement at age sixty-five!

Richard's story points up one of the issues with which industry is wrestling today—should the retirement age be flexible or automatic?

A logical beginning to a discussion of retirement policies would be an examination of present practices. Unfortunately, really good data as to the relative number of employees subject to compulsory and flexible retirement policies are hard to come by. In general though, there are about twelve million employees covered by pension plans, and over half of them are subject to automatic retirement at a fixed age—either sixty-five or sixty-eight.

In spite of this majority subject to automatic policies, however, you will find few industrialists who do not agree that in theory a flexible retirement policy is the ideal. The problem is in translating theory into effective daily practice.

There are three very strong arguments favoring a flexible approach.

First, are the demographic developments discussed by the previous speaker which have resulted in there being more and more older people. In this connection also, the whole thrust of medicine today is directed towards prolonging the productive life of the individual—eighty year olds with the durability of Richard will be commonplace in the fairly near future.

Second, is the medical fact that chronological age bears little relationship to physical age. In fact, it seems that we have many ages, a different one for each of our various organs. It follows then that retirement at any fixed age can bear little relationship to actual physical or mental disability.

The third argument is philosophical. There is, I believe, a major and most important trend underway in American industry towards a philosophy of what might be termed "individualism." In the first half of the twentieth century there has been a great reorganization of our economic life. Out of this have developed whole new patterns in the structure of our society—whole new communities of enterprise. This has occurred with such speed that we have lagged in the necessary understanding and skills to manage the human relations problems involved. Now, however, industry is becoming conscious as never before of the importance of treating each employee with the dignity, differentiation, and respect which is his due as an individual.

Thornton Wilder has written that, "In American thinking a crowd is not a homogeneous mass, but one and one and one." I believe this is true, but we are only now beginning to learn the techniques by which this philosophy can be effectively implemented in an industrial society. A flexible retirement policy would seem to be in harmony with this basic individualistic approach.

Arrayed against these three strong arguments are a number of very real practical problems. The importance of them varies widely from company to company. At some point, however, in many companies, their combined weight tips the scales in

favor of a compulsory or automatic system of retirement. Here are some of the more important of these practical considerations:

1. The size of the company. The larger the company, the more difficult becomes a selective and individualized approach.
2. The nature of the company. A telephone company, for instance, may employ hundreds of thousands of line men. It just isn't safe to have old men climbing poles in all kinds of weather. But the number of jobs to which a line man can be transferred is very limited. This kind of situation can make compulsory retirement almost a necessity. On the other hand, in a factory making small tools there is usually a fairly wide variety of light, sedentary jobs. Under these circumstances, job transfers from heavier to lighter work are more easily arranged.
3. The nature of a company's industrial relations. The greater the degree of employee confidence in a company, the easier becomes the administration of a flexible policy. The degree of informality of employee relations is another important factor. Many an industry is bound to inaction by its own red tape. Many collective bargaining agreements are so written as to make job transfers almost impossible.
4. The union attitude. This is often determined by the nature of the industry and of its employment prospects. For instance, the textile unions strongly favor a compulsory retirement policy because of the long-term downward trend in employment in this industry in some parts of the country.

It is the combined weight of these and similar considerations which incline many managements to the automatic approach.

There is no question, however, that the trend is towards more flexible arrangements. Both management and unions appear to be behind this trend and my guess would be that within five years or so only a small minority of plans would have the fully automatic retirement at age sixty-five which is today still quite prevalent.

Before leaving this area, it is perhaps worth mentioning that this trend to a flexible approach has given rise to a search for some kind of objective physical criteria which can be used to determine a fair and correct retirement age for each separate individual. Although this search for physical criteria for retirement is understandable, I feel that it misses the mark. We have men in our factory who by almost any imaginable physical standards would not only be unemployable, but dead. Yet these men are doing an acceptable and, in some cases, superior job. (Mind triumphs over matter to a truly extraordinary degree.) It would seem to me that job capacity or productivity is the only yardstick properly usable by a company as a criterion for retirement.

2. EXECUTIVE RETIREMENT POLICIES

The growth of compulsory retirement practices for industrial workers has been paralleled at the executive level. Here the additional incentive to make room for new blood is also an important factor in the thinking of many companies. The ideal of the young executive with great drive, dynamism, and ulcers still exists in many industries in this country. In the last year or so, however, the pendulum appears to have started in the other direction. One hears more and more about managements under which older executives are retained in an advisory capacity and as elder statesmen, so that their organizations can continue to benefit from that wisdom which comes only from years of experience. I believe this trend is more in harmony with the biological facts. Many men achieve a "plateau of usefulness" which can continue into the seventies.

3. PRERETIREMENT COUNSELING

A by-product of the wide spread existence of pension plans in industry, and of the increased possibilities for retirement which they create, is the current trend towards interest in the counseling of older workers. Here again it is not possible to give accurate figures as to the incidence of counseling programs in industry. One of the real problems in gathering such data is the difficulty in finding an adequate definition for "counseling." In general, though, it would appear that roughly half of all companies having pension plans are making some attempt at employee counseling.

Counseling is of importance regardless of whether a flexible or compulsory retirement policy is in effect. Although frequently associated with compulsory plans, I personally feel that it is probably of equal importance where a flexible policy is involved. This is for two reasons.

First, a flexible policy should not be regarded as the absence of a policy. A "too late" retirement, when health has already been seriously impaired, is as bad or worse for the individual as a "too early" retirement. We were jolted into recog-

nition of this when within a few months several years ago we carried three men out of our plant in a horizontal position as a result of heart attacks. Two were dead and the other severely disabled. So medical counseling is of very great importance.

Secondly, whereas under a compulsory policy a man knows and can plan for his retirement date, under a flexible plan it is all too easy for a man to fool himself into thinking that retirement is something to be thought about later—maybe next year, or the next, or the next.

The trend towards counseling is characterized by limited knowledge and unlimited enthusiasm. This seems to be due to the very human tendency to generalize on the basis of personal attitudes or experience. Because people vary so widely in their attitudes, good examples can be found to document any point of view which one may hold. As a result, the field is in a general state of flux. A good deal of experimentation is going on; and a good many quite contradictory reports are being made concerning the results. Many companies, seeing this, have understandably adopted a wait and see attitude.

In those companies which have programs, a variety of techniques are being used. Here are just three examples illustrating three different methods.

The Prudential Insurance Company uses fully qualified psychological counselors who follow a non-directive counseling method. It is applied both to individuals and to small groups of employees as well. This is typical of the more formal, professional approach.

Mrs. Wilma Donahue of the Institute for Human Adjustment, Division of Gerontology, at the University of Michigan has conducted courses for the older employees of several large industries in Michigan. The course consists of a series of perhaps nine weekly sessions attended by both husband and wife. Various areas of interest such as health, activities, finances, and so on are introduced by experts and then opened for general discussion. Mrs. Donahue herself provides the all important continuity and interpretation which gives the really lasting values to the course. Mrs. Donahue emphasizes a community approach to the needs of older workers.

A third method is followed by my own company. It is typical of the informal, individual type of counseling. It has two phases. One, an annual medical examination and counseling interview of all employees over age fifty by a highly qualified internist. The other, an informal interview—or rather "chat"—between the employee and the Personnel Manager. This latter is initiated at age sixty, repeated at age sixty-five, and then repeated again annually until retirement. It is very informal and non-directive in the sense that the discussion follows the individual's particular interests and problems rather than any set outline. This program has been in effect for about three years. During this period some thirty men have retired, all voluntarily. The Management Committee established to enforce the retirement of workers whose productivity had seriously dropped has never yet been called upon to act.

To return to the first phase, the annual confidential physical examination, an example of just one of its values may interest you.

A few months ago, while examining a piece-worker in our Grinding Department, the doctor discovered early signs of cancer of the rectum. The man had absolutely no idea that anything at all was wrong. Within a few days, however, surgery was performed. A month later he was back on the job. The surgeon recently told us that unquestionably this man's life had been saved through the early finding of the cancer. This fact in itself is sufficient gain for the Company. However, there is also an economic gain—in this case, the full value of a highly trained precision grinder with over twenty years of experience.

4. POST-RETIREMENT ACTIVITIES

A fourth trend in this field is that involving post-retirement activities. This is the newest of the trends—the baby. It is weak yet but shows great promise.

One of the interesting developments in this area is the organization of retired employees into groups or clubs. Both companies and unions have been studying their possibilities. The club with which I am most familiar is naturally our own. It is called the R.O.T.C.—The Retired Old Timer's Club. The group meets monthly at the plant and is involved in an increasingly wide range of manual projects, tours, outings, and social activities.

Such clubs have a terrific attraction to a substantial number of retired employees. Apparently this is because it helps them retain their identification with the company and their old surroundings. To many it is like coming home. These groups have other values too. They seem to help by taking a man's mind off himself as he helps others in the group. Active employees seeing their retired friends happy, healthy, and well adjusted, are influenced to look with more positive attitudes

towards their own retirement. Of course, the existence of such a group, is living proof of a company's philosophy of regarding its employees as individuals in whom it has a lasting interest, continuing beyond their usefulness as productive units.

A few companies have similar groups, but, as yet, the number is small. Curiously enough, I think that one reason for this is that the cost of such a group is so nominal. Many companies, and especially the larger ones, tend to fall into the error of measuring a policy's worth by the price tag attached. In doing so, they make a major mistake. It is often the little personal things that most affect a man's happiness and his attitude toward his company.

The thesis running through each of these four areas—flexible or automatic retirement, executive retirement policies, counseling, and post-retirement activities—is the imperative of a focus on the individual. The antithesis is the continued trend towards ever larger groupings of people for purposes of maximum productive efficiency. The synthesis is the development of a renewed emphasis on the dignity and potential of the individual in our society.

Today's industrial employee makes not just a living, but also a life at his work place. It is industry's task to seek to satisfy man's spirit as well as his material wants so that each may find in his work not only a good living, but a good life.

That which man makes is of no value unless it in turn makes man.

A FOURTEENTH-CENTURY COSMOLOGY *

GEORGE BOAS

Professor of History of Philosophy, Johns Hopkins University

In 1949 the Library of the Johns Hopkins University purchased a fourteenth-century Latin manuscript entitled *De Macrocosmo.* As far as has been discovered, the manuscript is unique, though there is some evidence that it was copied from a draft made by its author. This evidence consists of frequent repetitions of words and phrases, once in a while of whole lines, which have been eliminated by erasures. This would hardly occur if the original manuscripts had been dictated. If there was an author's original, it has disappeared to the best of our knowledge. The handwriting in the codex appears to be that of an Italian scribe of the fourteenth century, but the illuminations which introduce each book are most probably French and date from the beginning of the fifteenth century. This would not be unusual since itinerant illuminators visited all parts of Europe for just such purposes.

THE AUTHOR

Fortunately we know at least the name of the author and the title of the work. The Prologue begins, *Incipit liber Aurelii Mar* [*ci*] *Trivisano Veneti De Macrocosmo, i.e., De Maiori Mundi.* Later we are told that Trivisano was of the Parish of San Marziale in Venice. The work is unfinished and on the last page we learn that its composition was interrupted by the author's death in 1378. We learn also that when he was writing Book V he was fifty years old and that he was forty when he began the work (page 61 verso). In Book IV, when speaking of John Hestenden, he mentions the year 1347. We therefore can conclude that the book as a whole was written, let us say, between 1345 and 1378, though it is more likely that it was begun somewhat later. If we allow the author two years to write what he completed in Books V and VI, we might conclude that he was fifty-two when he died, which would make the date of his birth 1326. That is of course too precise, but we may set his life in the second and third quarters of the fourteenth century.

HISTORY OF THE MANUSCRIPT

As for the history of the manuscript, we have the following information.

(1) Giovanni degli Agostini in his *Notizie istoriche,* Venice, 1752, Volume I, preface, p. xlvii, writing of the library of Apostolo Zeno, says,

> Non meno delle altre scienze, sece figura tra'nostri la Filosofia si Platonica che Aristotelica. Volendo ammaestrare il suo figliuolo Lodovico, Marco Trivisano della Parrochia di San Marziale, compose un Libro, sotto titolo di Macrocosmo, o sia *de Majori Mundo,* che quantumque da X anni ne travagliasse d'intorno, nientedimeno colto da morte nel 1378, non potè a fine condurlo.

This would seem to show that our manuscript, or a copy of it, was in the library of Apostolo Zeno.

(2) How the manuscript came into Zeno's possession we do not know, but we have evidence that it remained possibly for a while in the hands of its author's heirs. In L. Romanin's *Storia documentata di Venezia,* Venice, 1855, Volume III, Book viii, Page 366, appears the following passage.

> Tre Trevisani, Marco, Bernardo ed Andrea, ce si presentano in questo secolo XIV come distinti cultori delle scienze naturali e filosofiche. Il primo, abitante nella parocchia di San Marziale e morte nel 1378, lasciò un' opera intitolata "Macrocosmo" o "De Maiori Mundo," scritta per istrusione di suo figlio Ludovico, nella quale trattava in sei libri delle meraviglie della natura, une specie di Cosmografia, che passata nel secolo scorso nella biblioteca di Apostolo Zeno, venne sciaguratamente a mancare prima che i libri di questo grande erudito pervenissero alla Marciana.

This does not show that Zeno purchased the

* This paper is a brief summary of the contents of a manuscript in the library of the Johns Hopkins University. Thanks to a grant of the American Philosophical Society, I was enabled to make a transcription of this manuscript. The original codex, a microfilm of it, and my typewritten transcription are now on deposit in the Johns Hopkins University Library where they may be consulted by scholars who are interested in fourteenth century science. The purpose of my summary is not to reproduce all the contents of the codex, which would be impossible without an extended translation of the mathematical proofs, but to indicate briefly its purpose and conclusions.

codex from the Trevisano family nor indeed how he acquired it, but in view of his own residence in Venice and the fact that the manuscript was in his possession, there is no reason to suppose that it left the city.

(3) At Zeno's death his library passed to the Marciana, as Romanin points out. In the Marciana there is a *Catalogo dei codici di Apostolo Zeno* (MSS. ital. Cl. CI, no. 285) which carries on the fly-leaf the notation that it was written by Marco Forcellini, "Zeno's collaborator in the last years of his life." The twenty-second entry runs as follows.

Liber Aurelii auctoris Marci Trivisano Veneti de Macrocosmo, i.e., de majori mundo. Comincia il Prologo: Quilibet elevata intellectui rerum ordinem speculanti: finisce*: gratias referans bonorum laugitovi* [obviously a misreading of *largitori*]. Indi comincia il Trattato cosi: *De divisione et compositione continui;* il quale è diviso in sei libri, e termina con queste *parole: productos fuisse planetas.* Trovasi poscià d'altra mano la sequente annotazione: *Adverse quisquis praesens opus inspexeris, quod Dominus Marcus Trivisano de contrata Sancti Martialis Venetiarum hujusmet operis auctor quippe Aurelius, id ipsum non complevit morte praeventus anno Domini millesimo trigentesimo septuagesimo octavo, cujus animam Iesu Christo feliciter commendare dignetur quiscumque in eo legevit Amen.* Codice membranaceo bellissimo, in foglio coll' effige dell' autore in abito patrizio nella lettera iniziale del primo libro, a con fregi di minio ed oro, a varii fiorami anche nelle iniziali degli altri libri. Il carattere è della fine del secolo quartodecimo.[1]

This description answers perfectly, with the exception of two misreadings of single letters, to that of our manuscript. Since neither the description of the manuscript nor the *incipit* has been found in the usual catalogues and lists of Italian and other *codices,* we are concluding that the Johns Hopkins *codex* is that which was owned by Zeno.

(4) This conclusion is somewhat fortified by the following.

In the catalogue of the Marciana appears a marginal note alongside the entry for the Trivisano manuscript: *Non trovato.* But at the bottom of the page there is a note in the hand of G. Valentinelli, formerly librarian of the Marciana, which reads

Questo codice XXII fu veduto a questi giorni in Torino, come me ne avverte il bibliotecario del Duca di Genova in Torino. Venezia 12 aprile 1865.

Whether it was seen in the library of the Duke or not is not recorded, nor is there any explanation of how it got to Turin. But there is every likelihood that it was stolen from the Marciana, or from the Duke's library later, since the top of page one has been snipped by a scissors, or some similar instrument, and it is probable that the cut was made to remove the library's stamp.

(5) In 1913 our manuscript was bought from a dealer, whose identity is unknown, by the father of Dr. George Rosenthal, now of California. How the Florentine dealer acquired it, we have no way of knowing. It passed from Dr. Rosenthal's hands into those of *L'Art Ancien* in Zurich and from that firm into the library of the Johns Hopkins University.

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An outline of the work as a whole is given in the Prologue. Following a tradition which appears to be Franciscan and is certainly Augustinian, Triviano takes as his Biblical authority for his main idea a text from *Wisdom,* "Thou hast ordered all things, O Lord, in number, weight, and measure." Under *number,* the author will discuss the great question of whether One—or Unity—is a number or that of which number is composed. By *weight,* he will mean magnitude. By *measure,* he will mean time, following Aristotle who says that time is the measure of motion. This accounts for the first three books. The fourth book, he says, will be devoted to the question of whether the world had a beginning in time or is eternal. The last two books will be an Hexaemeron. The Prologue ends with the customary profession of faith in Catholicism and the disavowal of any heretical or erroneous opinion which may have been unintentionally expressed.

BOOK ONE

The question of number is introduced as essential to an understanding of the nature of the continuum. The problem of the continuum is whether it is composed of infinitely divisible parts or of ultimates which are not further divisible. But before that question can be settled, the prior question of the nature of unity must be raised. And the basic problem of unity is whether One—or the monad—is actually a number or that of which number is composed. According to

[1] I owe this information to the kindness of my colleague, Dr. Edward Williamson, who transcribed it in Venice.

Aristotle, One was not a number. Trivisano is anxious to demonstrate that it is a number, for by a fusion of the meanings of *unitas*—the number One, the monad, the unit, the element—he hopes to show that points and instants are simply units with all the properties of the number One.

He begins then by differentiating in good Aristotelian fashion between actual and potential number. Actual number is that which can be named as odd or even, what we would call a cardinal number. Potential number cannot be named nor given *realiter*, for example, the square root of 15. A second distinction in numbers must be made between those which are *numerati*, as when we assign a number to external things; those which are *numerantes*, which themselves enumerate a set of things; those by which we count. This distinction is that of the function or uses which numbers have, but to Trivisano they correspond to the external object, the meaning of a symbol, and the "soul." A third distinction is imposed upon him by the philosophy of his time, the distinction between form and matter. The form of a number has to be considered apart from that which the number enumerates; one can speak of ten horses or ships or men, and the objects enumerated do not change the form of the decade. But when one speaks of number *materialiter*, one has to remember that certain matters cannot be enumerated, so that there appears to be a problem of what numbers can and what cannot be associated with certain subjects. As for unity, that can be spoken of in two ways, since both corporeal and incorporeal beings can be said to be units. Both are indivisible as far as their quantity is concerned, but the corporeal is distinguishable from other units by its "ultimate nature"; the incorporeal is neither divisible nor distinguishable. Trivisano then points out that since his interests is in *numerus numeratus* from the formal and not the material point of view and in corporeal unity, other considerations will be omitted.

He then moves on to list certain characteristics of such numbers in terms of which he will decide whether One is a number or not. These characteristics are commonplace. I shall simply list them since they illustrate Trivisano's method of argument. Numbers then are simple or prime, or composite; regular or irregular, the former of which has sides which are equal, the latter not;[2] long or linear, as Two, square or areal [*superficialis*], as Four; cubic or corporeal, as Eight. The first is made by "congregation," the second by "generation," the third by "production." Thus units aggregated in a single direction [*secundum longitudinem*] constitute a line; lines generate surfaces or areas; lines and surfaces produce bodies. Four corollaries follow: (1) every composite number is generating or generated or produced; (2) every linear number is regular; (3) every square number is areal and every cube corporeal; (4) every square number is both areal and regular and every cube corporeal and regular. But none of these statements are simply convertible. The other customary traits of numbers, evenness and oddness, and so on, given by Euclid in the *Definitions* introducing the seventh book of the *Elements*, he decides not to discuss, since they are sufficiently known to all *qui aliquantulum arithmetricam didicerunt*. He then sets down his thesis: Unity is a *numerus numeratus*, simple, the principle of all corporeal beings in itself, an indivisible minimum in the continuum, incapable of being less by quantity nor greater through rarity, and nevertheless distinguishable in spatial position.

He first proceeds to list his opponents beginning with Euclid who defines number as a multitude composed of units, Jordanus Nemorarius, "Algus," Algebra, whom he apparently thinks is a man not a subject, Boethius, Isidore of Seville, and Avicenna, all of whom repeat the usual formula that the unit is what composes number and therefore cannot itself be a number. Against such authorities he argues that since Unity has all the characteristics of number, it too must be a number, though a number of a unique type. The proofs that Unity is a number are perhaps not needed in a summary for modern readers, none of whom has any doubts about the matter, but it may be well to cite one proof for its historical interest. I give merely his demonstration that One is a prime number.

> Every number, he says, according to the mathematicians, is either a prime or composite. A prime number is one which is measured by unity alone, as appears in the seventh book of Euclid [VII, def. 11]. But since any number numbers [or enumerates] itself, as all concede, and there is no number which is enumerated by unity alone, except unity, as known by inspection, therefore unity alone is a prime number. But since according to all mathematicians any prime number is linear, and unity is a prime number, as has been concluded, it follows that unity is a linear number. And consequently unity is not only a number but also a prime and linear number.

[2] Trivisano makes a fusion between numbers and simple geometrical figures, as will appear shortly in the summary.

But One can also be shown to be a square and areal, a cube and corporeal.

This, however, is only the beginning of a series of demonstrations of the extraordinary properties of the number One. It is not only a square, but the first of the squares, a cube, and the first of the cubes. It is also an odd number and hence masculine. It is furthermore a "perfect" number, for it is equal to all its parts, since it has parts only *in potentia* and not *in actu*. Here it is clear that Trivisano is defining "parts" as factors. It is also "progressive" since it can be added to numbers to form a continuous series and can be the first number in a progression. To summarize the proofs of the other properties of Unity would be impossible, for the proofs are so detailed that they would have to be quoted *in extenso* to sound plausible. We can, however, cite the conclusion of the First Book which gives one some idea both of the properties which this number is believed to possess and also of the use to which Trivisano will put his findings.

From all these things it is obviously possible to affirm that Unity which alone among numbers has every perfection, since it is the center of all magnitudes which are odd and regular, for it is the first linear number and hence generating, the first square and hence generated, the first cube and hence produced, and therefore bears a resemblance to the primal creator. It has also the greatest conformity to the Trinity which is one in essence, three in persons. From Unity itself, as has been said, is composed all created corporeal nature. It is the first essence, though of three perfections, which shows and demonstrates as far as is possible the perfection of its Creator. Most great in other respects is the power of Unity by which all material created things are constituted and measured and seem to be distinguished from one another. For all material things are composed of an amalgamation of units and of numbers composed by such amalgamation.

Trivisano thus believes that he has not only established the thesis that One is a number and a number *sui generis* but that it is identical with the units out of which all nature is created. For, as we have said above, he takes such words as "square" and "cube" in both an arithmetical and a geometrical sense and the latter to him applies to the real physical world. Hence cubes are not merely a kind of number but also solid bodies. It is this fusion of meanings which makes it possible for him to construct his cosmology on a numerical or Pythagorean foundation. He thus appears as one of the early members of the school of physicists who maintained the possibilities of finding mathematical expressions of all physical laws.

BOOK II

The main problem of Book II is whether the continuum is infinitely divisible or not. Opinion on this question is stated to fall into two schools: on the negative side are Plato and Democritus and "some few moderns," on the affirmative, Aristotle and Algazel and some of their followers. Trivisano himself maintains that the continuum is composed of atomic parts, further indivisible. He will break up his exposition into three sections, in the first giving the reasons of Aristotle and Algazel, in the second those of Plato, Democritus, and himself, in the third replying to the arguments of his opponents. He proceeds immediately to Aristotle's reasons for believing the continuum to be infinitely divisible.

In the fifth and sixth books of Aristotle's *Physics*, it is maintained that continua are such things as those whose ultimates are one and undivided. But the ultimates of the indivisibles are not one. Therefore continua are not made up of indivisibles. Indivisibles lack ultimates since it is one thing to be ultimate and another to be that of which a thing is an ultimate. Therefore, if an indivisible had an ultimate part, it would be either divided or divisible. Aristotle's second reason is that if the continuum were composed of indivisibles, then either the whole would be in contact with the whole or a part with a part. But the indivisible does not touch a second part since it has no parts. Therefore, it must touch the whole. If then two indivisibles are in total contact, they will not have distinct places and consequently would not constitute anything bigger than themselves. The third reason is physical. If the continuum were composed of indivisibles, then the indivisible would be divided. But this implies a contradiction. Three suppositions underlie this proof. First, that at any time something may be moved more quickly or more slowly; second, that the quicker traverses more space in equal time than the slower, third that it is possible that the time measuring the motion of the slower is in the proportion of one and a half times the time measuring the motion of the faster and this is taken in respect to its magnitude.

The faster can be moved through a certain magnitude in a time composed of six instants by the first and second supposition. Therefore the slower could cover

the same ground in a time composed of nine instants by the third supposition. Just as the total time is to the total magnitude, so the mean time is to the mean magnitude and contrariwise. But the swifter can traverse the mean magnitude in a time composed of three instants. Therefore the slower will traverse the mean of its magnitude or be moved in a time composed of four and a half instants and thus the indivisible will be divided.

Algazel's reasons are six in number. (1) Let there be three indivisibles composing a continuum of which one is between the two others. Then one indivisible on the extremes will touch the middle indivisible or one will be the same as the other or not. If it is the same, then one of the extreme indivisibles will penetrate the middle indivisible and thus the three indivisibles will be the same. And in the same way one can prove that a thousand indivisibles will fuse into one. If, however, one extreme touches the middle indivisible and the other does likewise, then that middle indivisible must have parts and the indivisible will be divided. (2) Let there be a magnitude composed of seven indivisibles and let there be two mobile indivisibles on the ends which begin to be moved at the same time with equal velocity until they meet. But they can only meet in the middle. Then one of them will cut the first part and the other the other and consequently the same middle indivisible will be divisible. (3) If the continuum were composed of indivisibles, it would follow either that two indivisibles moving in opposite directions with equal velocity would not be able to pass through each other or that the indivisible would be divided. (4) It would also follow that the diameter would be equal to the side. But this is impossible.

(5) Let a rod be placed in sunlight and let us imagine a line coming from the center of the sun passing through the end of the rod to the extremity of the shadow. When then the sun moves in the sky, the shadow moves too, and the extremity of the line touches the shadow. I then ask whether, when the center of the sun is moved by one indivisible, the extremity of said line is so moved or not. If it is moved, it either traverses a part of a point on earth, which is impossible, because a point has no parts, or it traverses a point. And if it does, then while the center of the sun moves one point in the sky, the extremity of the shadow will traverse an equal space on earth and equal points make equal magnitudes. Therefore, while the sun traverses a given space in the sky, the extremity of the shadow will traverse an equal space on earth, which is impossible. For the sun traverses thousands of thousands of miles while the shadow moves only the distance of a single hair. If now the extremity of the said line is not so moved in relation to the sun's motion, then there would be one line having two extremities in relation to the same part, that is, the first point from which it is moved and the second at which it has arrived. Which is false.

(6) Let there be described a circle about the center of a wheel. Then it is asked whether when the wheel is revolved through one indivisible the circle described by the revolution is described by the greater or by the lesser [i.e. by the center or the circumference]. If by the first, then there will be as many indivisibles in the little circle as in the big one; if by the second, then the indivisible will be divided.

To these arguments, all of which think of the indivisible as extended in space, are added two arguments of Scotus and one of Gregory of Rimini, which take the same for granted, as well as a half a dozen others which Trivisano adds for good measure. As they all are based on premises which are common to those which we have summarized or quoted, there is no need to repeat them here. They do illustrate the preoccupation which men of this time had with the application of geometry to solid objects or to physical questions, as well as the ingenuity with which they fabricated and attempted to solve the paradoxes which this subject entailed. It would be perhaps more profitable to consider the arguments by which it is demonstrated that the continuum is composed of indivisibles.

Though the doctrines of Plato and of Democritus agree, continues Trivisano, in maintaining that the continuum is composed of indivisibles, they differ in that Democritus believes the indivisibles to be corporeal, whereas Plato held that bodies were composed of surfaces, surfaces of lines, lines of points. There are six ways of proving that the continuum is composed of indivisibles: (1) the way of "essential dependence," (2) that of "virtual coherence," (3) that of "resolution," (4) that of "diminution" (*minoratio*), (5) that of "measure," (6) that of the motion of the continuum.

(1) Essential dependence implies an order. There can be no order without posteriority and priority. Hence just as there must be a first cause in the order of causes, so there must be parts if there is to be a whole. Similarly the continuum must be an arrangement of something and that something cannot logically be infinitely divisible.

(2) Every whole composed of an infinite number of parts, as the cubit is composed of two half-cubits, is

a magnitude infinite *in actu*. But no continuum is a magnitude infinite *in actu*. Therefore no continuum is a whole composed of an infinite number of parts. Nor consequently can it be divided nor be divisible into infinite magnitudes.

It does have a potentially infinite number of parts, but not an actually infinite number. The argument here rests on Trivisano's assumption that the component members of a continuum must be discrete, an assumption natural enough in his time.

(3) The argument from "resolution" depends on the assumption that the continuum has no "proper parts," i.e., there is no more reason to cut it here rather than there. In this way it differs from a unit of measure which can be resolved into smaller units, as the yard is resolved into feet, and feet into inches. The continuum can be divided potentially in almost any manner, there being no *a priori* rule determining the divisions. Their order is not that of priority and posteriority—in spite of the proof above from essential dependence—but of composition. A body, for instance, can be potentially black and white and indeed other colors indifferently, though actually it must be one or the other of the possibilities. Therefore it is held that the continuum, however divided, will be resolved into a finite number of component members, though in accordance with another principle of division the finite number may be larger or smaller. But whatever the principle of division, there will be indivisibles in that system of relation.

(4) Everything from whose essence something can be taken as from a whole, has that something as an essential part. But one can take an indivisible from the continuum as from a whole. Therefore the continuum has the indivisible as an essential part. Trivisano cites as an example of this two straight lines which cross each other. Do they or do they not touch in some part? They obviously do and that part is an indivisible point. And therefore that point is an essential part of the line and the line—a continuum—must be composed of indivisibles. Moreover, if the point were subtracted from the line, the line would be broken. It would then lose its essential quality. It is to be noted that Trivisano believes that the point of intersection of the lines is common to both lines.

(5) If the continuum were infinitely divisible, the whole world could be contained in one small cube like a die. For one could slice off the die a thousand thin slices which one could then lay end to end and slice them into another thousand and so on until the slices enclosed the world. This seems impossible to Trivisano.

(6) The argument from motion runs as follows. If a line can be moved, then the point which terminates the line must be moved. But everything which is moved changes its location. Therefore the point changes its location. But Aristotle maintains that the indivisible cannot be moved and uses the motion of a line to prove the impossibility of a continuum's being composed of indivisibles. Therefore Aristotle is wrong. This result is so shocking that Trivisano feels that he must expatiate at length upon it, bolstering his simple demonstration of the mobility of points with other proofs. These are omitted here since most of them employ the same presuppositions as those summarized or are of so compact a nature that they would require translation at length.

The purpose of the rest of Book II is not merely to prove that the continuum is composed of indivisibles, but that these indivisibles are instants of time as well as points of space and that these two can be assimilated to the concept of unity. In order to accomplish this purpose it is necessary to examine various puzzles involved in the contact of lines, the divisions of time, the motion of concentric circles, and the like. As the text shows these puzzles and apparent paradoxes were multiple and evidently commonplace of the period. Trivisano, though he examines them at length also points out that there are many more which he does not want to weary his readers with. If he can fuse the concept of the Unit with that of the point and the instant, it is clear that he can attribute to these latter entities the almost miraculous properties of the first. This book, therefore, is not only useful as a demonstration of the kind of arguments which were used in the fourteenth century to prove physical theorems—or if one prefer, metaphysical theorems—it is also a kind of anthology of the opinions which men of this time held about the spatial and temporal continua.

BOOK III

The question now arises of whether the world has always existed or whether it had a beginning in time. And if it had a beginning, when did it begin? And how did it arise? Was it made by motion and transmutation or without these by emanation? Did the Prime Mover produce it eternally? Such are the questions which will occupy Trivisano in the third book.

He divides the authorities on this subject into the following groups.

(1) Aristotle, Averroes, and their followers. These maintained that the world is eternal, never having been created nor produced from anything pre-existing.

(2) Avicenna and Algazel. These maintained that the world had no beginning in time but was eternally produced by God, not created.

(3) Moses and the Church, and Plato. The world was created by God *ex nihilo* or out of eternally pre-existing matter.

(4) Democritus and Leucippus. The world was formed out of pre-existing atoms, infinite in number. No single world now existing has existed from eternity, but there are infinite worlds and always will be.

(5) Anaxagoras, Empedocles, and Heraclitus. There is one world, but it has not existed from eternity. But there was motion in the cosmic matter before the world was made.

The common assumption of those who posit an eternal world is that nothing is made of nothing, since everything that is made must be made of matter. Trivisano then proceeds to set forth the views of the various ancient philosophers, following Aristotle, in order to illustrate this presupposition. He takes up first those who posited the pre-existence of one kind of matter and then those who posited that of several. This whole section of the book is a good example of what knowledge men of the fourteenth century had of ancient philosophical beliefs.

After a discussion of the various elements of the material world, the theories of the Pythagoreans, and those of Plato, needless to say, as expressed in the *Timaeus*, Trivisano restates the principle question of the book, whether the world has existed from eternity or not. The argument here is purely dialectical.

> Either God could have made the world from eternity or He could not have. The second alternative is heretical and therefore we accept the first. And if He could always have made it, then either He always wished to do what He could have done or He did not always wish to do what He could do. If the former alternative is true, then He always produced the world from eternity, which is contrary to fact. If the second, then he changed from not wishing to wishing, which is contrary to faith since God is not mutable.

The Scriptures, the Fathers, and the Saints have always maintained that the world did have a beginning in time. The problem is to explain the possibility of this. Trivisano, therefore, sets forth in three sections the opinions of the various philosophers for the eternity of the world; then the reasons of the theologians against this thesis, then a refutation of the first. This refutation is built up in twenty-one arguments. The arguments and refutations are equally dialectical.

Thus it is argued that if time had a beginning, and the world with it, then there would have to be a moment of time at which time began. That moment would be the last moment of the time before time began and the first moment of time as well. Similar arguments would follow from the assumption of a last moment in time. Therefore, in order to avoid an infinite regress—and progress—we assert the eternity of time and consequently of the world. Such arguments were common to metaphysics until Kant developed his antimonies of the pure reason. Trivisano is alive to all the difficulties of belief in the eternity of the world and also to those inherent in creation. In order to avoid them he distinguishes between generation and creation, and correlatively between corruption and annihilation. Generation presupposes matter out of which or into which the generated thing is generated. But creation is a term applied to that whose inception requires no matter whatsoever. So corruption is applied to that which leaves something material behind it after it ceases to be, but annihilation is applied to that which leaves nothing whatever behind it. Thus God can be said to create the angels and not to generate them. Similarly He annihilates but does not corrupt the angels. Similarly to exist from eternity must be understood in two ways. First it is said of that which has been from all time and it is in this manner that faith holds the world to be eternal. Second it is said of that which has continuous duration whether temporal duration or not—what would probably be called in modern philosophy the "timeless." This distinction is applied to all apparently temporal terms, such as "at sometime," "prior," and the like. Thus it would be false to say that God was before the creation of the world, for that would imply that time existed before there was any time. But it would make sense to say that God was, though the world was not, for He possessed a kind of duration outside of time.

From the nature of God and creation, as well as from the nature of time and eternity, six corollaries are said to follow which contradict the position of Aristotle, that the world is eternal. These corollaries are (1) that it is possible that something proceed immediately from and be made by God without motion and transmutation; (2) it is

possible that something be made *de novo* without its maker undergoing mutation; (3) creation is neither motion nor mutation; (4) creation is not mutation but is the dependence of the caused on the cause by which it was instituted; (5) creation is without succession (i.e., is a *totum simul*); (6) it is a property *in quarto modo* of God to create. The fourth mode of creating is given later in the manuscript where Trivisano distinguishes four senses of the verb *to create*. They are (1) to produce (*producere*), (2) to make (*facere*), (3) to bring to pass (*promovere*), (4) to make something from nothing, which Trivisano says is the proper sense of *to create*. God did not create all things *in quarto modo*, for though He created the Heavens and the Earth, he made the other corporeal things out of pre-existing matter.

BOOK IV

The program of the Fourth Book centers upon the actual age of the world and the time of the year in which it was created. Trivisano carries out his program by citing authorities among the *doctores sacrae scripturae*, the ancient philosophers, the astrologers, and the *computisti*. Most of these authorities maintain that the world was created when the sun entered the Ram and at the time of the vernal equinox, though others disagree with them. Thus Grosseteste asserts that the world was created in the month of *Nisan*, that is April, an opinion based on *Exodus*, ch. 12. That the world and time began at the vernal equinox is, however, founded on the belief of Grosseteste that this is the

> most temperate of times and the time of generation and that the equinoctial day has the special privilege among other days that the sun in its diurnal circuit of the earth illumines the whole earth and leaves no part of the earth when it shines upon it, whereas on other natural days the sun leaves some part of the earth without illumination, for a whole day without light.

The autumnal equinox will not do, since at that time "the sun is farther away from us."

The authorities whom Trivisano quotes in support of this thesis are the following. They are listed here as an illustration of the writers upon whom he relied.

Julius Firmicus
Hermes Trismegistus
Grosseteste
Rabanus Maurus
Peter Comestor
Vincent of Beauvais
Isidore of Seville
Servius [?] on Vergil's *Georgics*.
Richard Swineshead
Volterus [Walter of Odynton ?]
Al Bitrogi
Jerome
Bede
Ambrose
Basil
Damascenus

Opposing these authorities are the following.

Leicester [?] who in his *Computo* maintains that the world was created at the time of the autumnal equinox; Dionysius, "the Arabs and Egyptians," according to Vincent of Beauvais, and the Romans, according to Bede.

Some, however, place the beginning of the world in October. Among these are Roger Bacon, following Josephus. Still others put it in the summer, among whom are Julius Firmicus, Macrobius, and Al Bitrogi.

Trivisano then cites the opinion of John of Eschenden, that no one really knows when the world was created and no one ever will know unless it be announced to him by revelation. Trivisano prefers to agree, however, with the opinion that the beginning of the world took place at the time of the vernal equinox, though he agrees that Vincent's reason for assigning it to the autumnal equinox is *fortissima*.

The next question that is discussed is that of the ages of the world. Most authorities agree on there having been six ages. They disagree, however, on the length of each age. The first age ran from the Creation to the Flood, the second from the Flood to the birth of Abraham, a period whose duration is also a matter of dispute. The third age runs to the reign of David, the fourth to the destruction of the Temple or the Babylonian captivity, the fifth to the birth of Christ. Trivisano then states the various calculations of the Fathers, the Rabbis, and the Astrologers, and concludes that there is no certainty either about the length of each age or of the age of the world. One would need a divine revelation before attempting to be precise. Let this much, he says, stand for what human knowledge has achieved.

BOOKS V AND VI

The remaining books, as we have said above, are an Hexaemeron in the form of a vision. The author, after the usual protestations of orthodoxy

and disavowals of any opinions contrary to the teachings of the Church, proceeds to relate how he was transported to "that region called by the astrologers *Arim*," which later turns out to be really *Mira*. Here he meets with none other than Moses who relates the story of Creation as it proceeded from day to day. Moses, anticipating Josiah Royce's God, has a chronosynoptic view of history, and quotes the New Testament and even Aristotle when it suits his purpose. The value of the beginning of this Hexaemeron (Book V) to historians of ideas is again its gathering together of number-symbolism, and the supposed relation between arithmetic and geometry, eventuating in the attempt to elucidate the logical problem of the doctrine of the Trinity by playing upon the apparently paradoxical properties of cardinal numbers. The sixth book opens with an exegesis of the first verses of *Genesis*.

As a sample of his method, we cite his comment on the name *Deus*. "This name," says Moses,

> is given to the most eminent being for four reasons. For He is called God as if He were the Lord and this in Latin since he rules all. He is given the Hebrew name of Fear, for He should be feared by all. In the third place He is called in Greek, *Theos*, from *vision*, since He sees all. Fourth He is called *Theos* in Greek from *running*, because He runs through all things, not that He moves and changes, but that He bestows motion and change on all things.

This introduction leads into a theological discussion in which respect is paid to the Aristotelian argument for monotheism. The next question to be taken up is that of what creatures were first produced. The words, *heaven* and *earth*, raise problems of interpretation. Does *heaven* mean spiritual being and *earth* corporeal or are these words to be taken literally? What are the meanings of the words, *in the beginning* and *created?* Such are the problems which Moses attempts to answer. The answers are complicated since one has to consider not only all Biblical verses in which such words are used but also the writings of the Fathers and other ecclesiastical authorities. Then there are such puzzles as the shape of the heavens which God created. Was it spherical, hemispherical, square, triangular, hexagonal? Moses's answer is that it was circular, "because it is more perfect than any other figure." Moses also maintains that the circular heavens do not move. As for the earth, the element, earth, is spherical, but the Earth, as the habitation of mankind, is far from spherical, since its surface is marked by mountains and valleys, but nevertheless it tends towards sphericity in its shape. And this is proved by its shadow on the moon during an eclipse, as well as by the fact that the sun as it moves through the skies now illuminates one portion of the earth, now another. The perfect sphericity of the element, earth, is proved by its weight. "For if it is a simple element each of whose parts is heavy, it follows that each part seeks the centre and that consequently earth is perfectly spherical." The lack of perfect sphericity in the Earth is attributable, therefore, to its being a mixture of the element, earth, and other elements. The Earth finally is concentric with the sphere of the fixed stars.

The Earth does not however move. The hypothesis that it moves is made to save the appearance of motion in the heavens. But it cannot do so. For the sphere of the fixed stars itself moves and does not move in a regular fashion. Therefore the movement of the Earth would have to be irregular in order to explain the apparent movement of the sphere of the fixed stars. Nor could it revolve upon its own axis. For we see that bits of earth when removed from their natural position seek to regain it and when they are in their natural position, remain at rest. Were the Earth to revolve, it would have two contrary motions and that is in violation of natural law. And this is in accordance both with Aristotle and Sacred Scripture.

This suffices to give an idea of the technique of Moses's reasoning. What follows is a description of the work of the first three days and a fragment of the work of the fourth, at which point the treatise is broken off. Throughout Trivisano tries to reconcile the teachings of the Church, as he understands them, with those of natural science. He is convinced, as in the first four books, of the harmony between mathematics, physics, and theology. He accepts the traditional view of the chemical constitution of material objects, the doctrine of the four elements, as well as the usual views of the astrologers. If the Hexaemeron has been completed, it would perhaps have had its greatest value as a scientific *summa*, a compilation of the cosmological opinions of his times. As a source of material which is not usually found in modern libraries, it must be almost unique. It illustrates what knowledge a well educated man of university background would have known in Italy towards the end of the fourteenth century. Whether it contributes anything original to the

science of its time could be answered only by one more skilled than I in the literature of this period. But the very fact that it is not the exposition of a novel view is in itself of interest. For, if the opinion expressed about it in this paper is correct, it may be taken as a fair sample of learned opinion. Historians of fourteenth-century scientific thought, therefore, might do well to study it with this in mind, just as students of fifth-century thought would study Martianus Capella.

BABYLONIAN PLANETARY THEORY

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ABBREVIATIONS

ACT O. Neugebauer, Astronomical Cuneiform Texts (in preparation)
CCAG Catalogus Codicum Astrologorum Graecorum
JCS Journal of Cuneiform Studies
Kugler SSB Franz Xaver Kugler, S. J., Sternkunde und Sterndienst in Babel, Münster 1907–1924.
Neugebauer ESA O. Neugebauer, The Exact Sciences in Antiquity, Copenhagen-Oxford-Princeton, 1951
Pañcha Siddhāntikā by Varāha Mihira, trsl. G. Thibaut, Benares 1889
S.E. Seleucid Era
SSB See Kugler

PART I. THE PLANETARY THEORY IN GENERAL

INTRODUCTION

1. THIS paper concerns astronomical texts from Mesopotamia, written during the last three centuries B.C. The discussion is restricted to texts which concern the mathematical theory of the planetary motion, either in the form of "procedure texts" or as "ephemerides." The former can be characterized as collections of rules for the computation of the latter, which contain chronologically arranged predictions for planetary phenomena, more or less comparable to a modern "Nautical Almanac." I disregard contemporary observational texts and texts which utilize (in a fashion not yet fully understood in detail) the results of the theoretically computed ephemerides for calendaric and other purposes.[1] I also make no attempt to reconstruct the historical development which culminated in the theoretical texts as known to us mainly from the third century B.C. onwards. I do not think that the material available today is sufficient to justify such an attempt. All that we can do at present is to consider the procedure texts and ephemerides as a completed system of astronomical knowledge containing very few traces of its prehistory. The situation is thus about the same as in the case of the "Ptolemaic System" represented by the Almagest in A.D. 150 or the Old-Babylonian mathematical texts of the seventeenth or eighteenth century B.C.

2. The similarity between the astronomical cuneiform texts and the Almagest or the Babylonian mathematical texts is, however, restricted to the common difficulty in penetrating into the preceding phases of development. With respect to a real understanding of the given material we are in a far worse position with the astronomical cuneiform texts. I do not mean the inherent Assyriological difficulties, which are serious enough but which can be gradually reduced as our text material increases. Even if we had completely preserved texts and perfect linguistic understanding of their contents, we would be far from the level of insight that we have under similar conditions in the study of the Old-Babylonian mathematical texts. The theoretical reasons for the procedures that are followed by the mathematical texts are usually perfectly clear as soon as the text can be read. Elementary algebra or geometry suffices to penetrate to the motivation of every single step. Similarly, Ptolemy has given us a perfect insight into the methodology, empirical and theoretical, of his great astronomical work. In the cuneiform astronomical texts, however, only small areas are really well enough understood to enable us to follow the reasoning of the ancient astronomers. To a certain extent the theory of the lunar

[1] *Cf.* A. Sachs, A classification of Babylonian astronomical tablets of the Seleucid period. JCS 2: 271–290, 1950.

ephemerides is investigated sufficiently far to appreciate the motives for the major steps and to reconstruct in general the empirical basis upon which they are built. In the planetary theory, however, the progress of the past years has mainly consisted in bringing into clearer relief the extent of our ignorance. The days have passed when one could hope to find in the planetary theory something like the two major "systems" of the lunar theory (based either on discontinuous step functions or on linear zigzag functions[2]). We now know not only that each planet required theoretical considerations of its own, but that a much greater variety of systems and subsystems existed than was ever expected. For almost none of these procedures can the theoretical background be given for their characteristic differences, nor do we know upon what empirical data such fundamental elements as the zones of different synodic arcs were built. The final criterion for the "understanding" of an ancient scientific theory must lie in our ability to repeat its processes and to derive its results following its own methods. We can solve today, practically step by step as a scribe in 1700 B.C. would have done it, a problem concerning the division of areas, leading to quadratic equations. And an eclipse can be predicted exactly in the way that Ptolemy or Theon did proceed. But if we have a cuneiform tablet with planetary data for a given year, we are still unable to explain how they were obtained. To speak about Babylonian planetary theory is not much more than a convenient euphemism.

3. When Kugler about fifty years ago started the investigation of the planetary theory, the number of texts available to him was small enough to give the impression of a relatively restricted number of well defined methods, for which Jupiter seemed to be the foremost example. Slowly more and more texts for all the planets became known and began to increase the number of variants. Consequently, after the period of first discovery of the major principles by Kugler has come to a close, the next step must be a systematic collection and publication of all the available texts. It is almost twenty years since I started to prepare an edition of the "Astronomical Cuneiform Texts" available to me. Especially through the investigations of A. J. Sachs, it is now clear that all these texts came from two major archives, one in Uruk, one in Babylon. The Uruk archive was scattered all over the world, since about 1910, by antiquity dealers and excavators. The bulk of the Babylon archive, which was discovered earlier, reached the British Museum in the 1870's and 1880's, though some smaller batches ended up in Berlin, Philadelphia, and New York. It was only through the untiring efforts of Father J. Strassmaier, S.J., that the material copied by him in London was made useful to Epping and Kugler and, later on, to Schaumberger, Sachs, and myself. Almost all the work on Babylonian astronomy between 1880 and 1952 was based, directly or indirectly, on Strassmaier's copies, made between 1880 and 1892. The "Strassmaier era" drew to a close when, in the summer of 1952, Sachs was given access to about 1500 sheets of copies of astonomical texts made by T. G. Pinches in the British Museum in the 1890's, before the termination of his official activity at the museum. Many of Pinches' copies duplicate Strassmaier's, but the former are in a final form, ready for publication,[3] while Strassmaier copied only in a preliminary fashion for identification and later study. Pinches often succeeded in making joins between fragments which Strassmaier had recorded singly. Furthermore, many of Pinches' copies concern new texts, which Strassmaier never copied, about forty belonging to the group which I am discussing here. Since the steady, though irregular influx of new texts has again and again made it necessary to postpone the publication of my edition, I feel that at least a summary should now be given of the results concerning the theory of the planets, which is so much more affected by new discoveries than the lunar theory.

In concluding these introductory remarks I wish to stress once more the fact that all our present studies of the planetary theory can hardly achieve more than the disentanglement of the different procedures of computation, and classifying them into systems and subsystems in order to obtain secure texts. It is only after the completion of this preliminary work that one might be able to penetrate successfully to the theory as it was conceived by the astronomers who devised the rules which we slowly reconstruct from the fragments of ephemerides and procedure texts.

[2] For this concept *cf.* the example given below, p. 69.

[3] A volume of such copies by Pinches will be published in the near future by A. Sachs (through Brown University Library).

BASIC CONCEPTS AND DEFINITIONS

4. The most "natural" formulation of a problem usually constitutes the most difficult approach. Disappearance and reappearance of the lunar crescent is the most striking phenomenon for the moon, and upon it the calendar was built. To make the problem of first and last visibility the central problem of the lunar theory means, however, to start at the most complicated end instead of investigating first the problem of daily motion regardless of the question of visibility. It is this shift of emphasis which constitutes the great progress from Babylonian to Ptolemaic astronomy.

The Babylonian planetary theory follows exactly the pattern of the lunar theory, though perhaps in a less refined fashion. At the center of interest stand again the problems of first and last appearance, to which are added oppositions (though probably not in the exact geometrical meaning, but as appearances on opposite sides of the horizon[4]) and stations. All these are striking phenomena to the observer of the nightly sky, but all are ill-defined, affected by climatic and physiological conditions and the result of a great number of causes, the isolation of which was extremely difficult before an insight into the general cinematical conditions of the planetary motion was obtained. Here too, the progress made is evident from the Almagest, where, after the theory of the planetary motion is completed both for longitude and latitude, the problems of visibility appear as a simple by-product, determined by the geometrical and optical conditions near the horizon.[5]

5. The Babylonian approach is exactly opposite to Ptolemy's procedure. We know from a few cuneiform tablets that the problem of day-by-day motion of the planets was not ignored completely, but it is quite clear that it was considered merely as a problem of interpolation between previously established fixed phenomena like first and last visibility of Mercury as evening star, etc.

How these characteristic phenomena were found will be the main subject of our discussion. At the moment I shall only introduce a notation which I shall use from now on, replacing technical terms like heliacal rising or setting by certain Greek letters as follows:

A. Inner planets:
- *a.* Morning star: first appearance: Γ; station: φ; disappearance: Σ
- *b.* Evening star: first appearance: Ξ; station: ψ; disappearance: Ω

B. Outer planets:
- first appearance: Γ
- first station: φ
- opposition: Θ
- second station: ψ
- disappearance: Ω

An inner planet is invisible at inferior conjunction between Ω and Γ, and at superior conjunction between Σ and Ξ. An outer planet cannot be seen between Ω and Γ. Its "opposition" Θ probably has to be taken in the above-mentioned sense of rising at sunset.

6. It is the first objective of the Babylonian theory to find times and longitudes for these "Greek-letter phenomena." Time is always expressed in calendar years of the Seleucid Era, the twelve or thirteen months of the lunar calendar, and (in the majority of cases) in lunar days which are 1/30 of a mean synodic month. Longitudes are always counted in the ecliptic, by zodiacal signs and degrees. As far as is known at present, latitudes were ignored in the planetary theory. It is this fact which is perhaps the most striking indication of the more primitive level of the planetary theory as compared with the lunar theory, in which the latitude plays a major role.

The years of the Seleucid Era (abbreviated S.E.) are arranged in ordinary years and leap years according to the well known nineteen-year cycle as established by Kugler.[6] This rule of seven intercalations in nineteen years came into use between 383 B.C. and 380 B.C. This is demonstrated by the fact that five contradictions are now known[7] for the four cycles before 383, whereas no contradictions are found from 380 on. Thus my attempt,[8] made in 1946, to trace the origin of the nineteen-year cycle back to a period shortly after 480 B.C. is now definitely disproved.

[4] Incidentally, also the Greek term for opposition is still "evening rising" though combined in fact with 180° elongation. *Cf.*, e.g., P. Mich. 149, XI, 9–11.

[5] *Cf.* Almagest XIII, 7.

[6] Kugler, SSB 1: 212.

[7] Several of them established by Sachs in unpublished texts.

[8] The "Metonic Cycle" in Babylonian Astronomy. *Studies and essays in honor of George Sarton*, 435–447, N. Y., Henry Schuman, 1947.

The same holds for Kugler's hypothesis of a preliminary eight-year cycle.

The months of the Babylonian calendar are always real lunar months, the first day of which begins with the first visibility of the new crescent. The exact prediction of this phenomenon is the main problem of the lunar theory as known to us from about 250 B.C. onwards.

Because of the irregularity in the distribution of lunar months of thirty or twenty-nine days, respectively, the planetary theory is based on approximate dates. Each month is divided into thirty parts or "lunar days," assuming a mean length of each month. These lunar days were first discovered in planetary ephemerides by Pannekoek[9] and by van der Waerden.[10] Recently I have found them also in lunar texts (e.g., for the variation of the latitude). Because exactly the same units are fundamental in Hindu astronomy,[11] which shows many relations to Babylonian astronomy of the Seleucid period, I call these units by the Sanskrit term tithis (abbreviated 1ᵀ). It seems plausible that the transmission of the concept from Mesopotamia to India went through the intermediary of Greek astrology, which has left significant traces in all Hindu astronomical literature. The same holds for the other borrowings from Babylonian astronomy that will be mentioned below.

7. The use of ecliptic coordinates, based on the twelve-division of the zodiac, is undoubtedly also of Babylonian origin, transmitted by the Greeks in Hellenistic times to Egypt and finally to Hindu-Islamic astronomy. Thanks to the fact that the Babylonian lunar calendar was always strictly lunar in character, one is able to equate dates of the Seleucid Era with an error of perhaps ± 1 day to Julian dates. Much less secure are the longitudes, since it is certain that the lunar texts of System A and B place solstices and equinoxes at the tenth or eighth degree of their respective signs. On the other hand, Kugler and others[12] found that planetary longitudes as given in Babylonian texts of the Seleucid period show longitudes only about 5° or 3° greater than the longitudes computed with modern tables, in which the vernal equinox has the longitude zero.[13]

This apparent contradiction in the definition of longitudes disappears if one analyzes more closely the concept "vernal point" in Babylonian mathematical astronomy. All that is really known is this: (*a*) System A of the lunar theory assumes equality of length of daylight and night when the solar longitude is ♈ 10°; for all other solar longitudes a simple linear scheme of rising times is used in order to determine the length of daylight. Similarly, System B places the equinox at a solar longitude of ♈ 8° while the scheme of rising times is slightly refined in comparison with System A.[14] (*b*) In computing the duration of visibility of the new crescent, the problem arises to determine the time difference between sunset and moonset for given longitudes. This problem is equivalent to determining the rising time of a given arc of the ecliptic and is solved by using exactly the same scheme which is known from the determination of the length of daylight.[15] (*c*) If λ is the Babylonian longitude of a planet, λ' its value according to modern tables, then experience suggests that approximately

$$\lambda = \lambda' + 4° \pm 1°.$$

Facts (*a*) and (*b*) are mutually consistent and can be stated as follows. If λ_0 is the solar longitude at equinox, then System A assumes $\lambda_0 = 10°$, System B operates with $\lambda_0 = 8°$. This exhausts what is known by using Babylonian sources exclusively. If we then introduce modern computation, which leads to the result (*c*),[16] we have only established that System A located

[9] Calculation of dates in the Babylonian tables of planets. *Koninkl. Akad. Wetensch. Amsterdam, Proc.* **19**: 684–703, 1916.

[10] Zur babylonischen Planetenrechnung. *Eudemus* **1**: 38, 1941.

[11] *Cf.* recently Olaf Schmidt, On the computation of the Ahargana. *Centaurus* **2**: 140–180, 1952.

[12] Kugler, SSB **2**: 514–524; van der Waerden, *loc. cit.*, p. 45.

[13] Only in passing, it may be remarked that the same situation prevails during the first centuries of the Christian era in the astrological literature. As I have shown in my investigation of Demotic planetary tables (*Trans. Amer. Philos. Soc.* NS **32**: 230, 1942), there is a difference of about 4° for Saturn in the period between Augustus (−16) and Hadrian (130). Sharper results can be obtained from the dated references in Vettius Valens (40 instances) which show for the longitude of the sun deviations which decrease between A.D. 50 and 150 by about 1½°, as is to be expected because of precession. These results can be confirmed from some two hundred horoscopes which I have dated. The detailed investigation of the deviations for each individual planet are not yet completed, but sun and lunar nodes clearly show the same tendency. This holds at least to the sixth century A.D.

[14] *Cf.*, e.g., Neugebauer, ESA, 153.

[15] *Cf.* Neugebauer, The rising times in Babylonian astronomy. JCS **7**, 1953.

[16] It is irrelevant for the present discussion whether (*c*) is really reliable or the margin of error is, say 2° or 3°.

the vernal point (using modern terminology!) at about $\lambda' = 6° \pm 1°$, System B at $\lambda' = 4° \pm 1°$.

In other words, all that we may really conclude from the existing evidence is an error of about five days in the Babylonian determination of the vernal equinox. Such an error is not at all surprising in view of the difficulty of such determinations, as is well known from the history of this problem in antiquity. We know, furthermore, that Babylonian astronomy was satisfied with an exact four-division of the seasons as far as solstices and equinoxes are concerned, with the summer solstice (and *not* the vernal point) as the fixed point.[17] Finally, it is evident that equinoxes and solstices play only a secondary role in the lunar theory in order to determine the hour of sunset or sunrise during the year. These moments are always rounded off to full 10°, thus it was of very little importance to refine the scheme for the length of daylight.[18] But there is no trace of any definition of the vernal point as the intersection of ecliptic and equator (which nowhere appears in Babylonian astronomy), nor is there any basis whatsoever for the assumption that the measurement of longitudes in general depended on the location of the equinoxes and solstices.

8. Having reached the insight that the Babylonian norms for the equinoxes have no bearing on the problem of planetary longitudes, we can at present do no more than state that the available evidence suggests a correction of about $-5°$ in order to pass from Babylonian longitudes to modern ones.

This is not the place to discuss the history of the counting of longitudes in detail. It must suffice to say that it is plausible to assume that longitudes were measured from some fixed star, a hypothesis which has been confirmed by Sachs' discovery[19] of a fragmentary tablet which relates longitudes with "normal stars." It is a program for the future to reach more accurate values for the necessary correction from these Babylonian sidereal longitudes to modern tropical longitudes. To this end, the vastly increased textual material will have to be compared with modern computation after a clear distinction between ancient observational data and computed data has been reached. It is perfectly possible that different norms must be used for different classes of texts. Since the following discussion is concerned exclusively with mathematical theoretical texts and the ephemerides which were computed accordingly, the problem of reduction to modern longitudes does not arise. All longitudes referred to in the rest of this paper are to be understood as Babylonian longitudes, without knowing exactly how the zero point had been localized.

9. We still must present a few formal definitions. The Systems "A" and "B" in the lunar theory are not only distinguished by their different methods and norms for the computation of the length of daylight and night, but also by different schemes for the solar anomaly. System A assumes a discontinuity of the solar velocity at two given points of the ecliptic, with constant values elsewhere. In System B the solar velocity varies according to a linear zigzag function.[20] We shall extend this terminology to the planetary theory. We call a procedure "System A" when it is based on step functions, i.e., functions of constant value on fixed arcs of the ecliptic, with discontinuous changes at given points. The use of linear zigzag functions is characteristic for a "System B."

We shall have to mention several "subsystems" and "variants" of these procedures. For example, a simple system A may operate with two zones only. A subsystem A′ may then exist which is based essentially on the same two zones, but with small intermediate steps at the discontinuities. Similarly there often exist variants with rounded-off parameters or similar small numerical modifications of the principal scheme. It is often quite arbitrary which method is considered the primary one and which the variant; thus no weight should be attached to this terminology.

In the lunar theory, System A is customarily considered to be the representative of a somewhat earlier development as compared with System B, though in the preserved material both Systems are represented during the same period. I am no longer convinced that the actual situation is that simple, since several irregular lunar texts have come to light. For the planetary

[17] *Cf.* Neugebauer, Solstices and equinoxes in Babylonian astronomy during the Seleucid period. JCS **2**: 209–222, 1948.

[18] This is confirmed by the fact that the dates of the solstices and equinoxes were regulated by the 19-year cycle (235 lunations = 19 years) when at the same time more accurate period relations were used in the lunar theory, where the basic relation is 1 year = 12;22,8 months or 2783 lunations = 225 years.

[19] A late Babylonian star catalog. JCS **4**: 146–150, 1952.

[20] *Cf.*, e.g., Neugebauer, ESA, 105f.

theory, however, it seems certain to me that no evolutionary principle can be connected with A and B. The number of contemporary systems and variants is much too great to make any historical arrangement possible. This is confirmed by procedure texts which often contain on the same tablet rules for several variant procedures, A, or B, or subsystems. It is probably only expediency or required accuracy or personal preference which determined the choice of one method or another in computing a planetary ephemeris.

All references to texts in this paper are based on the numbering in my above-mentioned edition which is under preparation. The numbers are chosen such that the character of a text can be concluded from the first digit as follows:

300 Mercury	600 Jupiter
400 Venus	700 Saturn
500 Mars	800 Procedure Texts

MEAN SYNODIC MOTION

10. The fact that the Babylonian astronomers considered the determination of the characteristic phenomena as the prime objective of their planetary theory had far-reaching consequences for the whole structure of their methods. This becomes manifest if one observes that the single phenomena (henceforth denoted by the Greek letters according to the scheme on p. 62) were isolated from the rest of the orbital motion and treated as if they were independent celestial bodies, each endowed with a law of motion of its own.[21] For example, in one theory of Mercury it is assumed that if one appearance as morning star (Γ) falls into the arc between ♌ 1 and ♑ 16, then the next will have a longitude 106° farther ahead;[22] if, however, Γ falls in the next zone (from ♑ 16 to ♉ 30), then this "synodic arc" from one Γ to the next is 141;20°. This procedure (obviously of System A type) is entirely independent of the motion of the planet between Γ's or of the other phenomena which occur while the planet moves from the first Γ to the next. Thus one might speak of an independent "motion of Γ" which proceeds in exactly the same fashion as the motion of the sun in the lunar theory A, with the synodic arc taking over the role of the solar velocity.

11. However independent the treatment of the Greek-letter phenomena may be (for Mercury, e.g., we have four: Γ, Σ, Ξ, and Ω), they must always satisfy certain conditions which make them significant points in the real movement of the planet. For example, the disappearance Ω of Mercury as evening star must always precede in time the reappearance Γ after inferior conjunction. On the other hand, the longitudinal motion from Ω to Γ should be retrograde for the major part of the ecliptic, but slightly forward in ♈ and ♉. Conditions of this type, sharpened by numerical limits, obviously impose severe restrictions upon the rules according to which the single phenomena are made to proceed. To this must be added conditions of a purely practical type. The coefficients which regulate the transition from zone to zone must be reasonably small and convenient sexagesimal fractions, longitudes and dates should not require units less than seconds, etc. We are very far from being able to reconstruct the empirical data upon which all these requirements were based or even from being in a position to imitate the procedures which lead to the final arithmetical rules. One can appreciate the complexity of conditions to be satisfied only if one once has tried to derive the rules of an unknown system of computation from a badly damaged ephemeris. I have spent weeks of trial and error with such numerical samples before I was able to determine correctly the point of discontinuity between two zones of already known constant synodic arc. For the Babylonians to have developed these methods *ab ovo* from some empirical data is certainly one of the most astonishing achievements in the history of ancient astronomy.

12. There is, however, one evident condition which must be satisfied: if for a given planet one of the phenomena again returns to the same longitude after a number Π of occurrences, then every other phenomenon must have the same period. Otherwise the planet would not set exactly as often as it rises. The distances between individual settings and risings may, on the other hand, perfectly well fluctuate; and, indeed, the whole purpose of the theory is to produce such fluctuations according to the empirical facts, but they must never be allowed to fall completely out of phase. Consequently a "mean period" and a "mean synodic arc" must

[21] The nearest analogue to this approach can be found in the treatment of the lunar nodes in Hindu astronomy—perhaps not accidentally.

[22] At least in principle, if we disregard corrections which do not concern us at the moment, caused by the influence of subsequent zones.

be established and be in common for all phenomena of the planet.

Because of the independence of such mean values of the specific phenomena, we may choose arbitrarily any one of them, e.g., an appearance, and then formulate a common period relation, e.g., for all phenomena of Saturn, in the single statement that

$$256 \text{ appearances} = 265 \text{ years}$$

and similarly in other cases.

We now must define these mean values more accurately. Let us suppose that an appearance (Γ) takes place at a longitude λ_0, the next Γ at $\lambda = \lambda_0 + \Delta\lambda$, where $\Delta\lambda$ is the shortest arc between the two consecutive Γ's, counted in positive direction. According to this definition, $\Delta\lambda$ is always less than 360°. We call it the "synodic arc" from one Γ to the next. Let us now assume that after Π appearances, Γ returns for the first time exactly to λ_0. Then the total of the Π synodic arcs which lie between the first and the next appearance at λ_0 is a multiple $Z \cdot 360°$ of complete rotations of Γ. Then we call

$$(1) \qquad \overline{\Delta\lambda} = \frac{Z \cdot 360}{\Pi}$$

the "mean synodic arc." Obviously $\overline{\Delta\lambda}$ is the distance between equidistantly spaced Γ's such that their number remains Π and their total motion is a multiple of 360.

Having defined the mean synodic arc by (1) we now can ask how many arcs of this length can be placed on one circumference of 360°. The answer is obviously given by

$$(2a) \qquad \frac{360}{\overline{\Delta\lambda}} = \frac{\Pi}{Z}.$$

We call this number

$$(2b) \qquad P = \frac{\Pi}{Z}$$

the "period" corresponding to the relation (1) which also can be expressed by the equivalent statement

$$(3) \qquad \Pi \text{ occurrences} = Z \text{ rotations}$$

of Γ. The last relation is capable of a direct empirical check. Under ideal conditions one might indeed find that two risings of a planet occur exactly at the same place with respect to a fixed star after $\Pi - 1$ occurrences at different places. In practice, however, one will not be able to establish exact recurrence, Π being a very large number, but one will find only approximate returns, expressible in the form

$$(4) \qquad \pi \text{ occurrences} = s + \frac{r}{s} \text{ rotations}$$

where $\frac{r}{s}$ is a reasonably small fraction of one whole rotation. From such an empirical result the form (3) is readily obtained through multiplication by s:

$$(5) \qquad \Pi = \pi \cdot s \quad Z = s \cdot s + r.$$

We find in our texts many examples of relations of types (3), usually with large numbers Π and Z, corresponding to many years. It would be utterly misleading, however, to draw historical conclusions from such data. Not only is it very likely that the majority of relations (3) were derived in the form (4) which may require only a very short interval of recorded observations, but we also must realize that the values of Π and Z may have been adjusted such that the resulting mean synodic arc (1) is convenient for numerical computation. It cannot be underlined strongly enough that no parameter found in procedure texts or in the computation of ephemerides can be taken at face value for chronological conclusions (as has happened unfortunately often enough) because all these parameters are the outcome of compromises between observational data, theoretical requirements, and computational technique. And we are almost entirely ignorant of the details of all these steps.

13. As an illustration of this situation can be quoted the theory of Mercury. There can be, of course, no doubt that the Babylonian astronomers were fully aware of the requirement that all single phenomena should have the same mean period. Nevertheless, we can demonstrate the existence of at least five different periods for Mercury, two for Γ and one each for Σ, Ξ, Ω. The corresponding relations (3) and periods are

$$(6) \qquad \begin{array}{rrl} 145\,\Gamma &= 46 \text{ years} & P = 3;9,7,49, \ldots \\ 2673\,\Gamma &= 848 \text{ years} & P = 3;9,7,38, \ldots \\ 1223\,\Sigma &= 388 \text{ years} & P = 3;9,7,35, \ldots \\ 1513\,\Xi &= 480 \text{ years} & P = 3;9,7,30 \\ 684\,\Omega &= 217 \text{ years} & P = 3;9,7,32, \ldots \end{array}$$

Obviously the only significant fact connected with these different numbers is the common value of the period: 3;9,7 plus a fraction, the variation of which is so small that it is of no influence on the

practical results. It would be equally absurd to conclude from (6) that the theory of Mercury was based on only 46 years of records or as much as 848 years. We are everywhere equally far removed from the preliminary steps which were totally obliterated in the final theory which alone we possess.

14. In (6) we have replaced the word "rotations" of (3) by the equivalent "years." This is permissible in the case of Mercury because this planet remains so near the sun that each rotation of Γ or any other phenomenon reflects a rotation of the sun or a (sidereal) year. In general, however, this need not be the case. Two consecutive phenomena (say, Γ) may be separated by a synodic arc $\Delta\lambda$ while the planet itself made a complete sidereal rotation in the meantime. This happens for both Venus and Mars. A similar situation holds for the sun. Even if we assume something that is only approximately true, namely, that the distance between sun and planet at appearance or at disappearance, etc. is always the same, even then a progress of Γ by $\Delta\lambda$ might mean a progress of the sun by $\Delta\lambda$ plus one, or even two complete rotations. This is in fact the case for all planets except Mercury.

All these cases can be treated at once by introducing two integers i and k (which might be 0, 1, or 2) such that the actual planetary travel $\Delta\Lambda$ and the actual solar travel ΔS corresponding to a synodic arc $\Delta\lambda$ is expressed by

(7a) $$\Delta\Lambda = \Delta\lambda + k\cdot 360°$$

(7b) $$\Delta S = \Delta\lambda + i\cdot 360°.$$

For Mercury we have $i = k = 0$, for Venus $i = k = 1$. For Jupiter and Saturn $i = 1$ $k = 0$, for Mars $i = 2$ $k = 1$. Thus for the inner planets $i = k$, for the outer planets $i = k + 1$.

If we assume that in (7) $\Delta\lambda$ represents the mean synodic arc, then we obtain from (1) p. 66 and (7a)

$$\Pi\cdot\Delta\Lambda = (Z + k\cdot\Pi) \text{ rotations of planet}$$

or, because each planetary travel of $\Delta\Lambda$ leads from one occurrence to the next,

(8a) Π occurrences $= (Z + k\cdot\Pi)$ rotations of planet.

Similarly, if we use (7b) and remember that for the sun each rotation means one year,

(8b) Π occurrences $= (Z + i\cdot\Pi)$ years

and from this

(8c) $(Z + k\cdot\Pi)$ rot. of planet $= (Z + i\cdot\Pi)$ years.

We now introduce the period $P = \dfrac{\Pi}{Z}$ and obtain from (8c)

(9) $(1 + k\cdot P)$ rot. of planet $= (1 + i\cdot P)$ years

which allows us to compute the mean length of the rotation of a planet. Table 1 gives the numerical values which can be deduced from the texts on the basis of these relations.

TABLE 1

		i	k	P	(1+kP) rot. = (1+iP) years	Π occ. = (Z+kΠ) rot. = (Z+iΠ) years
☿	Γ	0	0	$\frac{44,33}{14,8}$ = 3, 9, 7, 38, ...	1 rot. = 1 year	44,33 occ. = 14,8 years
	Σ			$\frac{2,25}{46}$ = 3, 9, 7, 49, ...		2,25 occ. = 46 years
	Ξ			$\frac{20,23}{6,28}$ = 3, 9, 7, 25, ...		20,23 occ. = 6,28 years
	Ω			$\frac{25,13}{8,0}$ = 3, 1, 7, 30		25,13 occ. = 8,0 years
				$\frac{11,24}{3,37}$ = 3, 9, 7, 32, ...		11,24 occ. = 3,37 years
♀		1	1	$\frac{12,0}{7,11}$ = 1; 40, 13, ...		12,0 occ. = 19,11 years
♂		2	1	$\frac{2,13}{18}$ = 7, 23, 20	1 rot. = 1; 52, 50, 51, ... years	2,13 occ. = 2,31 rot. = 4,44 years
♃	A, A', B	1	0	$\frac{6,31}{36}$ = 10, 51, 40	1 rot. = 11; 51, 40 years	6,31 occ. = 36 rot. = 7,7 years
	B			$\frac{2,24,49}{13,20}$ = 10, 51, 40, 30	1 rot. = 11, 51, 40, 30 years	2,24,49 occ. = 13,20 rot. = 2,38,9 years
♄		1	0	$\frac{4,16}{9}$ = 28, 26, 40	1 rot. = 29, 26, 40 years	4,16 occ. = 9 rot. = 4,25 years

15. To the mean synodic arc there corresponds a "mean synodic time" which, by (8*b*), is given by

$$(10)\quad \overline{\Delta t} = \frac{Z + i\Pi}{\Pi} \text{ years} = \left(i + \frac{1}{P}\right) \text{years}.$$

This, however, is not a convenient form in Babylonian astronomy. The "year" plays a secondary role since the lunar months are the real units of time.

In System A of the lunar theory the following relation between year and mean synodic months is used

$$(11)\qquad 1 \text{ year} = 12;22{,}8 \text{ months}$$

but it is also fundamental in System B[23] and finally in the planetary theory.[24] We can formulate (11) in a different fashion by introducing tithis[25] such that

$$1 \text{ month} = 30^{\tau}.$$

Then we obtain instead of (11)

$$(12a)\quad 1 \text{ year} = 6{,}11;4^{\tau} = 12 \text{ months} + 11;4^{\tau}.$$

We call the amount

$$(12b)\qquad \epsilon = 11;4^{\tau}$$

the "epact" and can consider the use of (12*b*) as the equivalent of (11).

We can now formulate (10) in tithis, which are the proper units for the planetary theory. Using (12), we find for the synodic time, measured in tithis,

$$(13)\qquad \overline{\Delta\tau} = \left(i + \frac{1}{P}\right)(6{,}0 + \epsilon)^{\tau}.$$

It is this relation which we find explicitly used, e.g., in a procedure text for Mars and which, applied to other planets, reveals the consistent use of the value $11;4^{\tau}$ for the epact in all parts of the planetary theory.[26]

16. Formula (13) leads directly to another formulation which is of importance for the practical computation of the dates in ephemerides. We separate the right-hand side of (13) into three terms

$$\overline{\Delta\tau} = \frac{6{,}0}{P} + i \cdot 6{,}0 + \epsilon\left(i + \frac{1}{P}\right).$$

The last term is a constant

$$(14a)\qquad c = \epsilon\left(i + \frac{1}{P}\right)$$

and can be computed for every planet by means of the values given in the table on p. 67. The second term

$$(14b)\qquad i \cdot 6{,}0^{\tau} = i \cdot 12 \text{ months}$$

is a mere calendaric term, indicating a change of year numbers (of course after taking into account intercalary years which contain thirteen months instead of twelve, as regulated by the nineteen-year cycle). Finally we have the term $\frac{6{,}0}{P}$ for which we can write, because of (2) p. 66,

$$(14c)\qquad \frac{6{,}0}{P} = \overline{\Delta\lambda}.$$

Thus we have found the relation

$$(15)\qquad \overline{\Delta\tau} = \overline{\Delta\lambda} + i \cdot 12 \text{ months} + c$$

where $i = 0$ for Mercury, $i = 1$ for Venus, Saturn, and Jupiter, $i = 2$ for Mars and c given by (14*a*). In other words: the mean synodic time equals numerically the mean synodic arc plus a constant c and a trivial calendaric term.

Strictly speaking, (15) is only valid for the mean synodic time and the mean synodic arc—we have used, e.g., (14*c*) which introduces the mean value of $\Delta\lambda$. In practice, however, the true synodic arcs are never so different from the mean values as to make (15) useless for the true motion. Thus in the majority of ephemerides[27] we find the dates computed from

$$(16)\qquad \Delta\tau = \Delta\lambda + c(+\, i \cdot 12 \text{ months})$$

i.e., by adding a fixed amount to the change in longitude in order to obtain the corresponding change in time.

TRUE SYNODIC MOTION

17. If the planets and the sun would move with constant angular velocity in the plane of the equator, the theory of mean values, developed so far, would be all that we needed. In fact all

[23] Neugebauer, Untersuchungen zur antiken Astronomie III. *Quellen und Studien zur Geschichte d. Math.*, Ser. B4, 312, 1938.

[24] van der Waerden, *loc. cit*, note 10, p. 43 for Mars and Saturn.

[25] *Cf.* p. 63.

[26] For possible exceptions *cf.* p. 79 and p. 84.

[27] An unquestionable exception to this rule is found in No. 300*a* (*cf.* below, p. 77).

bodies show anomalies of their own, moving near the ecliptic (which makes greatly variable angles with the horizon) but not exactly in the ecliptic such that additional effects are caused by the latitude.

While all these effects were neatly separated in Babylonian lunar theory, apparently nothing similar has been attempted for the planets. One of course did realize that there are great variations depending on the ecliptic but one tried to describe them by anomalies associated with the single Greek-letter phenomena. We shall not be concerned with the problem of how the combination of consecutive phenomena of different kinds, in their natural order, showed the proper effects. For the moment we will only discuss for a single phenomenon the methods by which deviations from mean motion were produced, maintaining nevertheless the basic period relations.

18. The problem that we face is this: if, e.g., Γ occurs at a point λ of the ecliptic, we want to know the synodic arc $\Delta\lambda$ which separates the next occurrence of Γ from the previous one. Experience shows that $\Delta\lambda$ varies with the longitude λ of the initial location of Γ. Let us assume that M is the greatest synodic arc observed, m the smallest value, and that they deviate by equal amounts from the mean synodic arc $\overline{\Delta\lambda}$ such that

$$(1) \qquad \mu = \tfrac{1}{2}(M + m) = \overline{\Delta\lambda}.$$

The use of linear interpolation is then a natural device for intermediate values, i.e., to operate according to "System B."

This method is indeed found in ephemerides of Jupiter and Saturn. It may be illustrated by an excerpt from an ephemeris[28] of the latter planet (for Ω) (table 2).

TABLE 2

	I	II		
	11, 37, 25	26, 45, 30		
0.	11, 25, 25	8, 10, 55	♍	0.
	11, 14, 40	19, 25, 35		
	11, 26, 40	0, 52, 15	♐	
	11, 38, 40	12, 30, 55		
	⋮	⋮		
	13, 38, 40	19, 57, 35		
	13, 50, 40	3, 48, 15	♑	
15.	14, 2, 40	17, 50, 55		15.
	13, 54, 45	1, 45, 40	♏	
	13, 42, 45	15, 28, 25		

The arrangement is the same which is familiar from lunar ephemerides: the first column contains the differences for the second such that, e.g., in line I,15 is listed the amount 14;2,40° by which ♉ 17;50,55 in II,15 exceeds ♉ 3;48,15 in II,14. These differences increase with constant difference 0;12° from a minimum after I,0 to a maximum after line 15, and decrease by the same amount towards I,0 and from I,16 on. The exact values of these extrema, found by linear extrapolation, are

$$(2) \qquad m = 11;14,2,30 \quad M = 14;4,42,30.$$

We say that column I forms a "linear zigzag function."

It is clear from the relation between the two columns that column I gives the true synodic arcs between consecutive phenomena (Ω) whose longitudes are then given by column II. The extremal values of synodic arcs are (2). The period P of the corresponding linear zigzag function is

$$(3) \qquad P = \frac{2(M - m)}{d} = 28;26,40 = \frac{4,16}{9}.$$

This shows that after

$$(4) \qquad \text{II} = 4,16$$

occurrences of Ω, the same sequence of synodic arcs will be repeated. From this are derived the othe relations which we have listed in table 1 on p. 67.

It is, of course, necessary that the period P of the true synodic arcs be identical with the period which holds for mean synodic arcs. Similarly one must require that the mean value of the true synodic arcs must be identical with the mean synodic arc $\overline{\Delta\lambda}$, or

$$(5) \qquad \tfrac{1}{2}(m + M) = \overline{\Delta\lambda}.$$

Because the period P can now be expressed either by means of the mean synodic arc (*cf.* p. 66) as

$$(6) \qquad P = \frac{6,0}{\overline{\Delta\lambda}} = \frac{2 \cdot 6,0}{m + M} = \frac{\text{II}}{Z}$$

[28] No. 703.

or from the linear zigzag function as

$$P = \frac{2(M-m)}{d} = \frac{\Pi}{Z} \tag{7}$$

we see that the extrema m and M and the difference d must satisfy

$$6{,}0 \cdot d = (M+m)(M-m). \tag{8}$$

This is indeed exactly the case for the parameters which we deduced from our example since

$$6{,}0 \cdot d = 1{,}12 = 25;18{,}45 \cdot 2;50{,}40.$$

It is clear that we are dealing with a consistent mathematical theory, the consequences of which were fully realized.

19. A variation of the synodic arcs according to a linear zigzag function which varies between given extrema is perhaps the simplest method of accounting for deviations from a mean synodic motion. Obviously, however, a strictly linear variation of the synodic arcs seemed too strong a schematization of the observed facts. Consequently, a different method was devised, in which the ecliptic was divided into sectors of different length, each of which carries a constant synodic arc ("System A").

Of course the values assigned to the segments of the ecliptic and to the corresponding synodic arcs are not fully arbitrary. We must again assume that the number of occurrences of the phenomenon in question during a certain period of time is empirically known, that is to say, we are given a number P which indicates how many synodic arcs fall in the mean on 360° of longitude. The same number P must be obtained whatever the distribution of ecliptic segments and synodic arcs may be.

Let us assume that we divide the ecliptic into s segments of length, $\alpha_1, \alpha_2, \ldots, \alpha_s$, respectively, thus satisfying

$$\alpha_1 + \alpha_2 + \ldots + \alpha_s = 6{,}0^\circ \tag{9}$$

and let $w_1, w_2, \ldots, w_s$ be the corresponding synodic arcs. We then see that $\frac{\alpha_1}{w_1}$ phenomena will fall on the first segment, $\frac{\alpha_2}{w_2}$ on the second, etc. Consequently the segments and velocities must be chosen in such a fashion that

$$\frac{\alpha_1}{w_1} + \frac{\alpha_2}{w_2} + \cdots + \frac{\alpha_s}{w_s} = P. \tag{10}$$

This method gives great freedom to reproduce empirical data about the observed values w_i of synodic arcs in certain parts of the ecliptic. In practice, however, this freedom is restricted by arithmetical requirements, e.g., that the ratios of consecutive synodic arcs should be convenient sexagesimal fractions.

As a simple example, I can again quote the theory of Saturn. System A for this planet operates with two segments only, of $\alpha_1 = 160°$ and $\alpha_2 = 200°$ length, and with synodic arcs of $w_1 = 14;3{,}45$ and $w_2 = 11;43{,}7{,}30$ respectively. The ratio of these two values is 6:5. From (10) we obtain

$$P = \frac{2{,}40}{14;3{,}45} + \frac{3{,}20}{11;43{,}7{,}30} = \frac{4{,}16}{9} = 28;26{,}40$$

in perfect agreement with (3) p. 69.

20. By means of System A a great variety of distributions of synodic arcs can be obtained. At first glance one might think that the synodic arc assumes only as many different values as there are different segments. This, however, is not the case. In the simplest possible case of two segments only, we have at least four zones of synodic arcs: two of the values w_1 and w_2 respectively, and two with linear increase and decrease between w_1 and w_2. This can be best illustrated in the case of Saturn, System A, which we have quoted in the preceding section. The segment of highest synodic motion $w_1 = 14;3{,}45$ begins at ♈ 0° and ends at ♌ 10. This means, however, that the last possibility for two phenomena, say Γ_1 and Γ_2, to be separated by an arc of length w_1 is the case where the longitude of Γ_1 is $\lambda_1 = ♍\ 25;56{,}15$ resulting in the longitude $\lambda_2 = ♌\ 10$ for Γ_2. If Γ_1 belongs to the arc from ♍ 25;56,15 to ♌ 10 the next phenomenon Γ_2 will have a longitude which is given by

$$\lambda_2 = ♌\ 10 + \tfrac{2}{3}(\lambda_1 + w_1 - ♌\ 10).$$

If, finally, Γ_1 coincides with ♌ 10°, then the above formula results in

$$\lambda_2 = ♌\ 10 + \tfrac{2}{3} w_1$$

which is the same as $\lambda_2 = ♌\ 10 + w_2$ since $w_2 = \frac{2}{3} w_1$. Thus the synodic arcs are constant only from ♈ 0 to ♍ 25;56,15, thereafter decreasing linearly to w_2 in ♌ 10. Here begins a new section of constant synodic arc $w_2 = 11;43{,}7{,}30$ until ♈ 0 − 11;43,7,30 = = 18;16,52,30. Then we again have linear increase towards $w_1 = 14;3{,}45$ at ♈ 0 (*cf.* fig. 1).

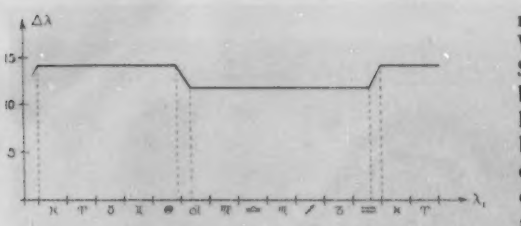

FIG. 1.

The situation can be still more complicated. In the case of Saturn the synodic arcs w_1 and w_2 are small in comparison with the length of the two segments α_1 and α_2. Thus at least for the major part of the first segment the synodic arc is w_1, of the second, w_2. In the case of Mercury, however, the synodic arcs are such that they often exceed the length α_i of the segments of the ecliptic. In such cases the values w_i need not appear at all explicitly as a synodic arc.

Let us consider, e.g., an appearance ♎ which occurs in ♌ 20 which belongs to the arc from ♍ 6 to ♎ 26 where a synodic arc $w_1 = 2{,}40$ is prescribed (*cf.* fig. 2, dotted line and p. 76). Adding 2,40° to ♌ 20 would bring us to ♑ 30, thus to a point 1,34° inside the next zone which extends from ♎ 26 to ♓ 10. Because the synodic arc in this zone is $w_2 = 1{,}46;40$, we must reduce the 1,34° in this zone in the ratio $w_2 : w_1 = 2:3 = 0;40$. Consequently the actual synodic arc for ♌ 20 amounts to 1,6° from ♌ 20 to ♎ 26 plus $0;40 \cdot 1{,}34° = 1{,}2;40°$ in the next zone; hence the total is 2,8;40° instead of $w_1 = 2{,}40°$. By considerations of this type it is seen that the influence of the "fast arc" from ♍ 6 to ♎ 26 is actually felt in a much wider zone (from ♈ 0° to ♎ 26) but with a considerably reduced maximum (*cf.* fig. 2).[39]

This shows how difficult a task it must have been to construct, from observational data, a law for the synodic arcs according to System A. The empirically found synodic arcs must be obtained as the result of properly limited segments α_i with coordinated ideal synodic arcs w_i such that (10) is satisfied and the combination of consecutive segments produces the observed amounts of synodic motion. It is certainly not surprising that many different attempts were made to solve this intricate problem and that the results were not necessarily strictly consistent. We will see, e.g., in the case of Mercury, two Systems A which operate on similar principles but do not lead to identical results. We do not know whether any final choice was ever made between such competing systems. The available evidence, however fragmentary, and our experience with the better known lunar theory, suggest the coexistence of multiple possibilities during the whole period from which ephemerides are preserved, i.e., for about three centuries.

[39] A similar situation for ☊ is shown in fig. 7, p. 78 where the dotted line represents the original step function for the synodic arc, the strongly drawn line the resulting actual synodic arcs as function of λ.

THE TEXTS

21. Before turning to the discussion of the specific situation of each single planet, I must give the reader at least a general impression of the character of the sources upon which our discussion is based. Above all, it is necessary to make clear that it is not "the" Babylonian planetary theory which we are describing here, but fragments of theories whose mutual relationship, historical setting, and practical application are unknown to us in the majority of cases. The reason for this is, of course, the lack of usable sources. I known of twenty-eight procedure texts for the planets—and, of these, only about ten are well enough preserved to be of real use. On the other hand, among the well preserved passages there are many duplicates, especially for Jupiter, which is also the planet best represented among the ephemerides (about forty are known). The ephemerides are in equally bad shape. We have 11 each for Saturn and Mercury, and nine and seven for Venus and Mars respectively, at least half of which are so badly damaged that they are unusable for our purposes. If I had to guess, I would say that

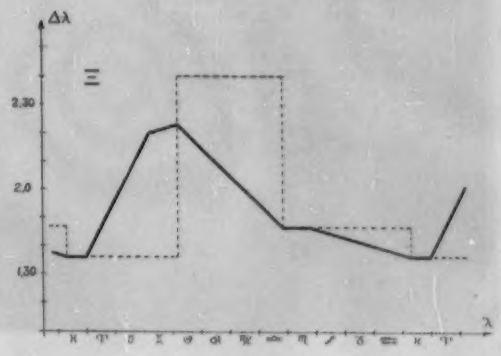

FIG. 2.

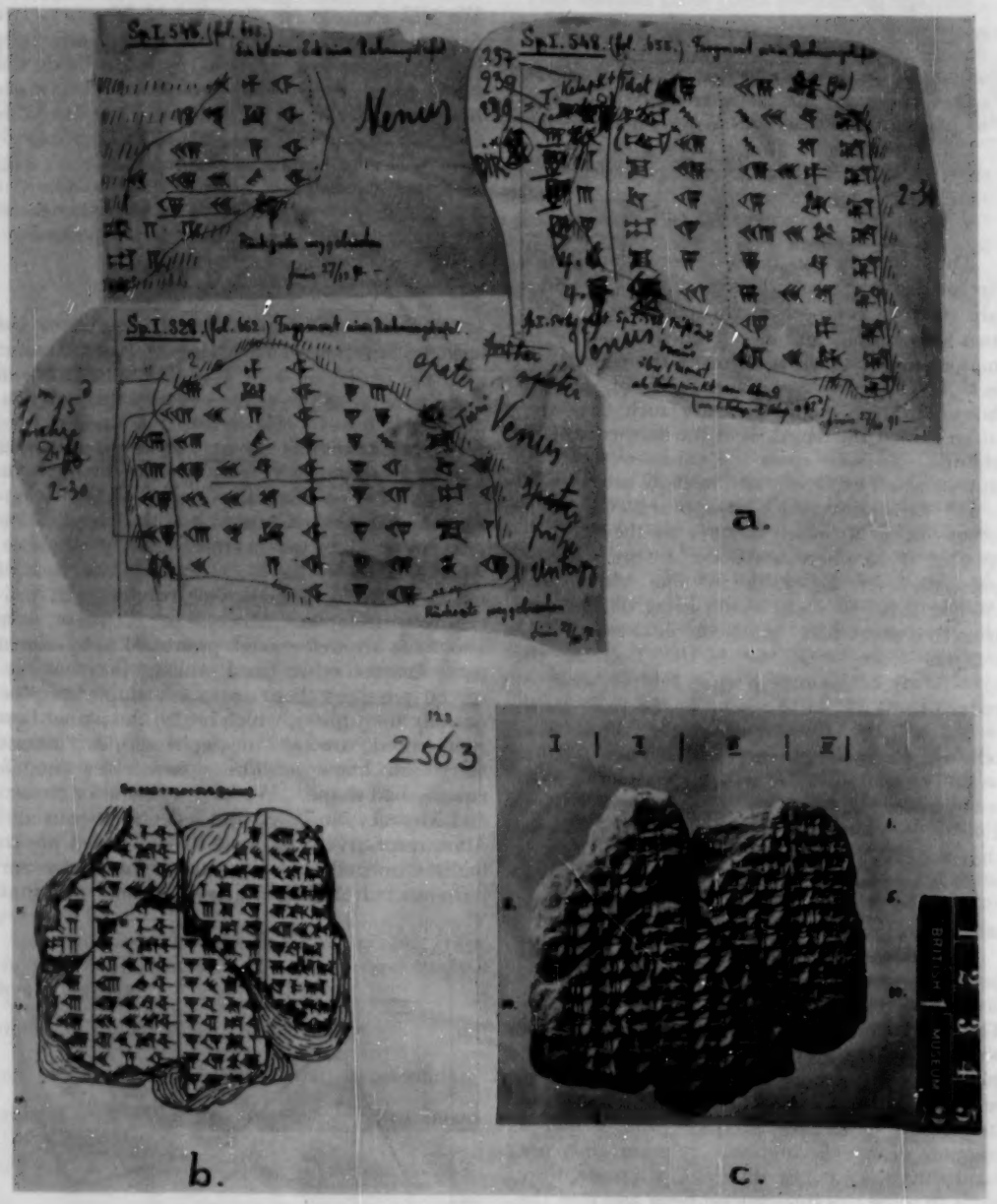

FIG. 3.

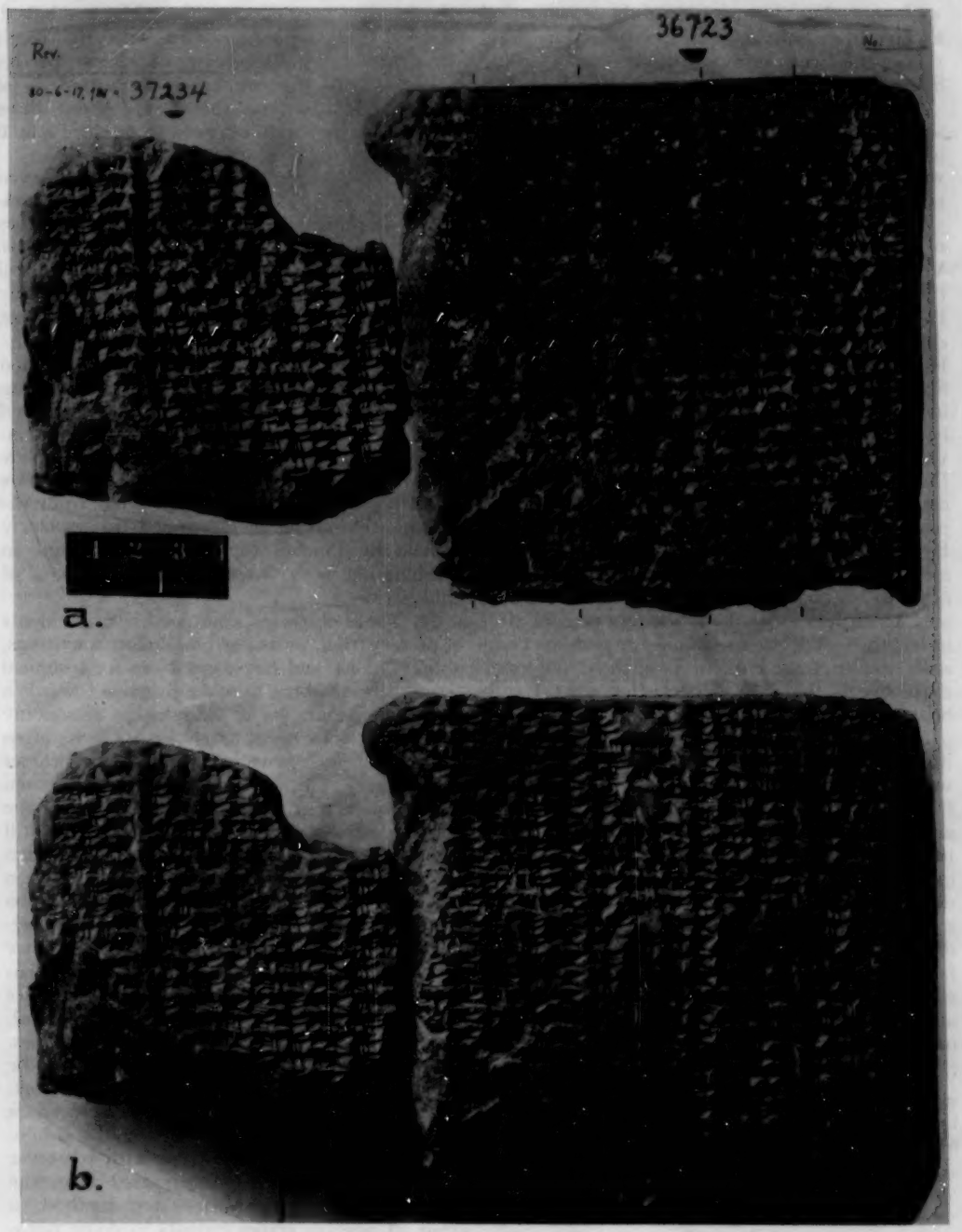

FIG. 4.

we have perhaps five per cent of the astronomical archive from Babylon and about the same fraction of the Uruk archive, always referring to the Seleucid period only.

22. The fragmentary character of our primary sources lends significance to every little accident in the preservation of the texts. A few examples will best illustrate the situation which one faces constantly in this kind of work.

We know especially little about the theory of Venus. Four fragments of an ephemeris for Venus were discovered by Strassmaier in 1891 in the Spartoli Collection of the British Museum. He copied them in his notebook and sent second copies to Father Epping; finally they reached Kugler. Three of these copies are reproduced in figure 3*a*.[30] Several years later Pinches realized independently that these three fragments could be joined to a bigger piece and copied them anew (fig. 3*b*). Kugler, having no knowledge of Pinches' results, published only one of the three connected fragments[31] plus a fourth disconnected piece,[32] both in normalized cuneiform writing. By using Pinches' joins and the detached fragments, one recovers parts of six columns of an ephemeris which originally had twelve columns and which was more than twice its present size in length.[33] Together with one, or perhaps two, still smaller fragments of a slightly different date, this is the only evidence for what I call System A_1 of Venus.

23. Another ever present difficulty with cuneiform texts is the problem of cleaning and preservation. Figure 4*a* shows the appearance of two fragments which recently came to my attention and which I joined after having realized that both were parts of an ephemeris of Mercury of great interest. The dates are from the first decade of the Seleucid Era (thus around 300 B.C.) and the system of computation is decidedly different from the method known so far.[34] These facts were established with great difficulty because of the incrustation of the tablets with salt and dirt, as is clearly visible, e.g., on the left and on the lower part of the larger fragment. After the join had been effected, the tablet was baked, cleaned, and rephotographed with the result shown in figure 4*b*, giving us almost perfect columns. All that we know of System A_2 of Mercury is based on this text. About three months of work were required to produce a final transcription of this new type of ephemeris.

It was a lucky accident that a well preserved text could be recovered below the salt incrustation. In most cases of this sort, the writing has been destroyed by the action of the crystals. Several times Strassmaier's copies show, as still preserved, passages which have crumbled away in the meantime. Without baking and cleaning, these texts will be destroyed slowly but surely. Figure 5*a* shows a text which has made good progress in the direction of decay. It is an ephemeris for Venus with a colophon followed by a procedure text which, when complete, contained the whole list of coefficients which were needed to compute the ephemeris. All that we know about this method is gathered from the traces still visible and copied in masterly fashion by Pinches (fig. 5*b*) 60 years ago, in combination with a few minute fragments of ephemerides.

24. These obstacles, combined with accidents of preservation, scribal errors, doubtful writings, abbreviations, and rare expressions for technical terms, all contribute to make progress towards a better understanding of Babylonian astronomy extremely slow. Even assyriologists have, often enough, a rather naive idea about the character of this work. It is, e.g., a widespread axiom among them that one can work only with the originals at hand. If this were so, we would still know absolutely nothing about Babylonian astronomy. I have made many joins between fragments in Chicago, Paris, Berlin, or London without ever having had the opportunity of seeing the individual parts physically. Year after year new fragments of the same tablet were identified as I slowly gained access to more copies made by Strassmaier, photos from Istanbul, texts in the United States, or Pinches' copies. The ideal method of work would, of course, be to have all the texts collected on one's desk. The next best (and the only practical) procedure is to utilize whatever is available: photos, copies, collations and to combine these with whatever theoretical insight can be extracted from the existing material. And the safest method to avoid all possible errors consists in doing nothing.

[30] The numbers "Sp. I. 545" etc. refer to the Spartoli Collection. The "fol. 655" etc. are references to the notebooks. Kugler added his notes in thick blue pencil, some corrections and remarks in ink.

[31] Sp. I 548 (SSB I Pl. 22).

[32] Sp. I 230.

[33] ACT No. 410.

[34] The join as shown in fig. 4*a* was obtained by pasting together the photos of the two fragments in the proper position and rephotographing them.

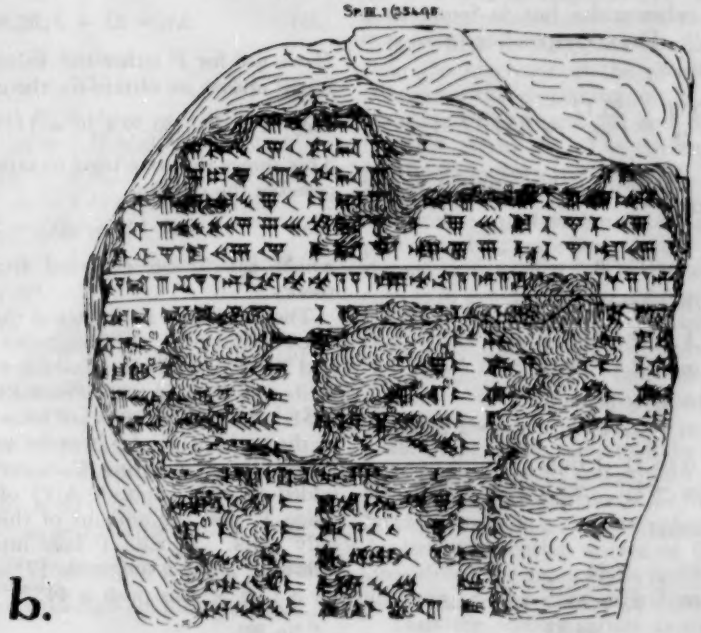

FIG. 5.

PART II. THE SINGLE PLANETS

The following gives a summary of what seems to me to be fairly secure in the theory of the planets as known to me today. I shall disregard severely incomplete fragments which could only be discussed within a detailed description of the textual evidence. This will be done in my forthcoming edition. The reader must realize, however, that the variety of fragmentary systems is still considerably greater than is apparent from what follows.

MERCURY

25. Four phenomena were considered for Mercury: rising (Γ) and setting (Σ) as morning star, rising (Ξ) and setting (Ω) as evening star. Two systems are known from ephemerides, both of type "A": one (A_1) considers Γ and Ξ as independent phenomena and finds Σ from Γ, Ω from Ξ, while the other (A_2) has the opposite arrangement, starting with Σ and Ω and finding from them Ξ and Γ respectively.

The periods of all these phenomena have been listed in (6) p. 66. The 46-year period is not attested from ephemerides but is found in a procedure text.[35] The corresponding mean synodic arc is given by

$$\overline{\Delta\lambda} = \frac{6,0}{P} = \frac{4,36,0}{2,25} = \frac{55,12}{29} \approx 1,54;12,24, \ldots$$

The last-mentioned ratio is also quoted in the Pañca Siddhāntikā.[36]

System A_1

26. For Γ we have three zones:

(A): from ♌ 1 to ♉ 16 thus
$\alpha_1 = 2,45°$ with $w_1 = 1,46°$

(B): from ♉ 16 to ♊ 30 thus
$\alpha_2 = 2,14$ with $w_2 = 2,21;20$

(C): from ♐ 0 to ♌ 1 thus
$\alpha_3 = 1,1$ with $w_3 = 1,34;13,20$

and hence the period

$$(1)\quad P(\Gamma) = \frac{2,45}{1,46} + \frac{2,14}{2,21;20} + \frac{1,1}{1,34;13,20} = \frac{44,33}{14,8} = 3;9,7,38, \ldots$$

[35] No. 800.
[36] XVII, 70.

Also for Ξ three zones are used:

(A): from ♎ 6 to ♒ 26 thus
$\alpha_1 = 1,50$ with $w_1 = 2,40$

(B): from ♒ 26 to ♈ 10 thus
$\alpha_2 = 2,14$ with $w_2 = 1,46;40$

(C): from ♈ 10 to ♎ 6 thus
$\alpha_3 = 1,56$ with $w_3 = 1,36$

thus leading to

$$(2)\quad P(\Xi) = \frac{1,50}{2,40} + \frac{2,14}{1,46;40} + \frac{1,56}{1,36} = \frac{25,13}{8,0} = 3;9,7,30.$$

The corresponding period of 480 years was also used in Greek astrology; *cf.*, e.g., CCAG 1 p. 163 ch. 51. For the resulting synodic arcs *cf.* figure 2, p. 71.

The time difference $\Delta\tau$ which corresponds to a synodic arc $\Delta\lambda$ should be found, according to a procedure text,[37] from

$$(3a)\qquad \Delta\tau = \Delta\lambda + 3;30,39,4,20^{\tau}.$$

which is rounded off in the ephemerides to

$$(3b)\qquad \Delta\tau = \Delta\lambda + 3;30,39^{\tau}.$$

If we use for P either the value $P(\Gamma)$ or $P(\Xi)$ found above, we obtain for the epact

$$(4a)\quad \epsilon = P\cdot 3;30,39,4,20 = 11;3,59, \ldots$$

This shows that one tried to satisfy as closely as possible the relation

$$(4b)\qquad \epsilon = P\cdot(\Delta\tau - \Delta\lambda) = 11;4^{\tau}$$

which should be expected from the general theory.[38]

The longitudes and dates of the settings Σ and Ω are found from the longitudes and dates of Γ and Ξ, respectively in a fashion which I have described in detail in these *Proceedings* **95**: 110–116, 1951. Several variants are known, but all agree in the main principle: in order to find, e. g., the longitude of a setting Σ, a certain "push" is added to the longitude λ(Γ) of the preceding rising Γ, and the amount of this push depends only on λ. If, e.g., Γ falls into ♈, then the following Σ has a longitude 12° ahead of Γ; but for a Γ in ♐ this push is 44°.[39] Similar pushes

[37] No. 801.
[38] *Cf.* above, p. 68.
[39] *Cf.* Neugebauer, The Babylonian method for the computation of the last visibilities of Mercury. *Proc. Amer. Philos. Soc.* **95**: 113, fig. 2 upper half, 1951.

are described for the dates. We finally know that occasionally a pair Γ, Σ or Ξ, Ω will remain invisible. This is the case when Γ falls between ♈ 10 and ♉ 20 or Ξ between ♎ 0 and ♏ 5.[40]

System A_2

27. Several features of System A_1 for Mercury were already known to Kugler. The existence of a parallel system, A_2, abstracted from an ephemeris which covers at least the years S.E. 4 to 22,[41] is entirely new and unexpected. The date itself is quite remarkable. Otherwise, the next earliest planetary text is an ephemeris for Saturn for S. E. 86 to 134[42] and the earliest lunar ephemerides are for S.E. 106 to 108 (System B) and for S.E. 124 and 125 (System A).[43] A fragmentary lunar procedure text[44] contains, as an example(?), references to S.E. 60 and 61, and similarly the dates for the consecutive phenomena of Jupiter for S.E. 61 and 62 are quoted in a procedure text for Jupiter.[45]

In the new text, Σ and Ω are taken as the primary phenomena, both computed with a four-zone system. Those zones are:

for Σ

(A): from ♋ 0 to ♍ 30 thus
$\alpha_1 = 1,30$ with $w_1 = 1,47;46,40$

(B): from ♎ 0 to ♁ 6 thus
$\alpha_2 = 1,36$ with $w_2 = 2,9;20$

(C): from ♁ 6 to ♈ 5 thus
$\alpha_3 = 1,29$ with $w_3 = 1,37$

(D): from ♈ 5 to ♊ 30 thus
$\alpha_4 = 1,25$ with $w_4 = 2,9;20$

for Ω[46]

(A): from ♋ 0 to ♐ 30 thus
$\alpha_1 = 3,0$ with $w_1 = 1,48;30$

(B): from ♁ 0 to ♌ 30 thus
$\alpha_2 = 1,0$ with $w_2 = 2,0;33,20$

(C): from ♌ 0 to ♈ 30 thus
$\alpha_3 = 1,0$ with $w_3 = 1,48;30$

(D): from ♉ 0 to ♊ 30 thus
$\alpha_4 = 1,0$ with $w_4 = 2,15;37,30$

Consequently the periods are

$$(5)\quad P(\Sigma) = \frac{1,30}{1,47;46,40} + \frac{3,1}{2,9;20} + \frac{1,29}{1,37} = \frac{20,23}{6,28} = 3;9,7,25, \ldots$$

and

$$(6)\quad P(\dot{\Omega}) = \frac{4,0}{1,48;30} + \frac{1,0}{2,0;33,20} + \frac{1,0}{2,15;37,30} = \frac{11,24}{3,37} = 3;9,7,32, \ldots$$

The dates are no longer computed by means of the simple formula (3*b*) p. 76 (which is, strictly speaking, valid for mean synodic arcs only) but by relations which are not accurately known since we have only rounded-off values at our disposal. It must suffice to say that the synodic times vary between $\Delta\tau = \Delta\lambda$ and $\Delta\tau = \Delta\lambda + 6^\tau$ and even $+9^\tau$. The amount to be added to $\Delta\lambda$ seems to depend on λ.

The reappearances Ξ and Γ are obtained by means of pushes from the preceding disappearances Σ and Ω respectively. Because of the incomplete preservation of the text, we know for Ξ only that its longitude is about 45° greater than the longitude of the preceding Σ when Σ falls into the beginning of ♊, increasing to about 1,20 in ♌ and ♍, and constant (58;30) from ♐ 20 to ♈. The corresponding pushes for the dates vary from 20^τ to 56^τ, with 30^τ from ♐ 14 to perhaps ♈ 10.[47]

Accurate information is obtainable for the pushes in longitude from Ω to Γ as indicated in the upper part of figure 6. It shows that Mercury is in general retrograde from Ω to Γ, except for Ω in ♈ and ♉. The corresponding times of invisibility from Ω to Γ are plotted in the lower part of figure 6 and are practically certain in spite of their being based on the analysis of rounded-off numbers only.

28. In principle, one should be able to combine the two Systems A_1 and A_2 into a single method of computing an ephemeris for Mercury. If we, however, take, e.g., in any ephemeris of System A_1 two consecutive values of Ω, we find that their difference generally is not what it would be according to the rules of System A_2.

Thus the problem arises as to how much the

[40] This is stated in the procedure text No. 801 and is confirmed by ephemerides.

[41] No. 300*a* with a small duplicate No. 300*b* for S.E. 10 to 18.

[42] No. 700.

[43] Nos. 100 and 1 respectively.

[44] No. 200*h*.

[45] No. 818.

[46] *Cf.* also fig. 7, p. 78.

[47] Similar pushes for longitudes and dates are listed in the Pañca Siddhāntikā XVII, 42–53 as if Systems A_1 and A_2 had been combined, though the general method is obviously the same.

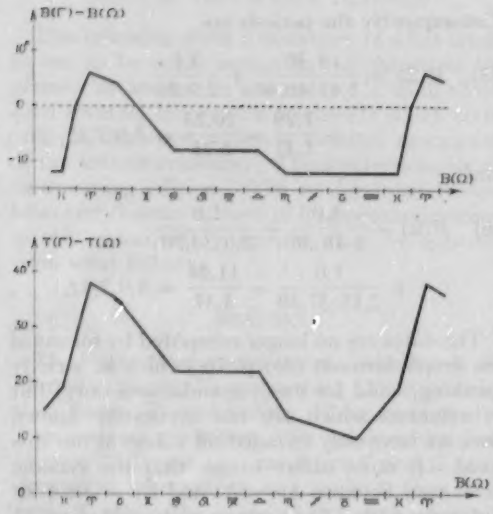

FIG. 6.

results of the two Systems might deviate from each other. We can investigate this question in the case of Ω. Assume, e.g., that $\Xi_0 = \otimes 6$. According to System A_1 the next Ξ will be $\Xi_1 = \mathfrak{M}\ 29;20$. Ξ_0 is followed by an Ω_0 whose longitude is found from Ξ_0 by adding the proper push, which amounts here to 43;12. Thus $\Omega_0 = \Omega\ 19;12$. Similarly we find $\Omega_1 = \mathfrak{M}\ 29;20 + 14;57,20 = \text{♐}\ 14;17,20$. This tells us that according to System A_1 the synodic arc for Ω in Ω 19;12 will be 1,55;5,20. On the other hand, we can compute the synodic arc for Ω directly from System A_2 and find for Ω 19;12 the synodic arc of 1,48;30, thus 6½ degrees less than before. One can carry out the same computation for all points in the ecliptic where Ξ or the pushes $\Xi \rightarrow \Omega$ change direction, being linear everywhere else. The result is shown in figure 7,[48] which demonstrates that deviations up to 10° are possible. We have no idea about the attitude of the ancient astronomers to this fact, which they must have realized since it would reveal itself immediately by computing a complete ephemeris once with A_1 and once with A_2. And we have equally little possibility of comparing these two methods with the actual facts, since we have no modern tables which are trustworthy for problems of visibility in Mesopotamia.

29. In all ephemerides, planetary as well as lunar, we meet the problem of initial values. In the case, e.g., of Mercury, System A_1, we can compute the columns for longitudes and dates of Γ if a single longitude and a single date are given. The same holds for Ξ; finally, all Σ and Ω are determined through Γ and Ξ. Similarly for System A_2. Thus a complete ephemeris for Mercury can be computed as soon as two longitudes and two dates are known.

How these initial values were found we do not know. Experience with the other planets suggests that also in the case of Mercury there were rules which permitted the transition from a phenomenon of one kind, e.g., Γ, to a phenomenon of another kind, perhaps Ξ. If this hypothesis is correct, then the problem remains as to how the one initial pair of a longitude plus the accompanying date was chosen.

Exactly the same question can be raised for all planetary ephemerides and for most of the columns in the lunar theory. In no case can a definite answer be given. Somewhere, of course, empirical elements must enter the picture, but it is by no means clear whether observational data were accepted without modification. Ordinarily the ephemerides contain numbers down to seconds—in other words, far beyond what could be guaranteed by direct observations. The plausible hypothesis that one started from some round values can be disproved, in some cases, by showing that the arithmetical scheme in question can never lead to numbers ending in ;0,0. In other cases, one can also demonstrate that simultaneous round numbers in parallel

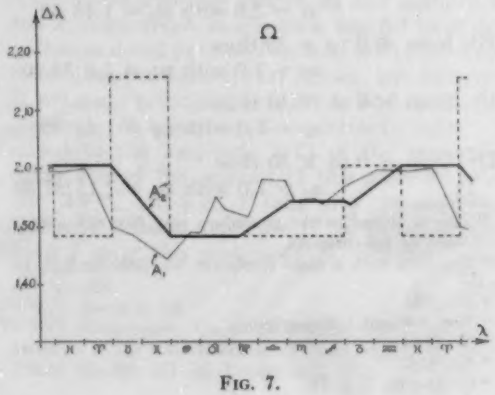

FIG. 7.

[48] The dotted line in fig. 7 represents the synodic arcs $\varpi_1, \ldots, \varpi_4$ of System A_2 on their respective segments of the ecliptic. The strongly drawn line gives the resulting synodic arcs. The irregular polygon drawn in a finer line indicates the result obtained in System A_1.

columns are excluded. In other words, the problem of initial values is at present entirely unsolved. This is only one aspect of the general fact that we are totally ignorant of the historical development of all methods in Babylonian mathematical astronomy.

VENUS

30. The phenomena which were tabulated in ephemerides of Venus are not only the risings Ξ and Γ and the settings Ω and Σ, but also the stations ψ and φ as evening and morning star respectively. The method of dealing with these phenomena is perhaps the simplest procedure in the whole planetary theory. We know from "System A_0," which is based on mean motion only, that the mean synodic arc was found to be

$$(1) \qquad \Delta\lambda = 3{,}35;30°,$$

a value which is also quoted by Varaha Mihira.[49] From (1) one derives an important relation, namely

$$(2) \quad 5\cdot\Delta\lambda = 17{,}57;30° = -\,2;30° \mod 6{,}0°$$

or; after 5 occurrences every phenomenon of Venus returns to almost the same longitude, namely, to a point only $2\frac{1}{2}°$ short of the initial place. In going from mean synodic arcs to true synodic arcs, one arranged five arcs in such a fashion that their total agreed with (2). In this way all phenomena of one kind were computed from one set of five initial values. This holds for all six phenomena under consideration. Finally there existed rules as to how one had to pass from one phenomenon of one type to one of the next kind.[50]

31. The result of using approximate values is visible in the method followed in the computation of dates. From the above value (1) of the mean synodic arc and from the general relation between solar motion and motion of Venus we see that the planet travels between two consecutive phenomena of the same kind in the mean the distance

$$(3a) \qquad \Delta S = \Delta\lambda + 6{,}0 = 9{,}35;30°.$$

For the corresponding time the rule

$$(3b) \qquad \Delta\tau = \Delta\lambda + 6{,}17;40^\tau = 9{,}53;10^\tau$$

is used, from which one can deduce a value $\epsilon = 11;3{,}4, \ldots$ for the epact. The accurate value $11;4$ of ϵ would be otained if $\Delta\tau$ had the value $9{,}53;11{,}28{,}40^\tau$, which shows that (3*b*) and perhaps also (3*a*) are rounded-off numbers.

Again, if we multiply (3*b*) by 5 and remember that by definition one mean synodic month is 30^τ, then we find

$$(4) \quad 5\,\Delta\tau = 49{,}25;50^\tau = 49{,}30 - 4;10^\tau = 8\cdot 12^m + 3^m - 4;10^\tau.$$

Since 8 lunar years ordinarily contain 3 intercalary months, one can express (4) in the loose form that the phenomena of Venus recur in a period of 8 years with the slight correction of $4;10^\tau$. If one would use the more accurate value of (3*b*) which we deduced from $\epsilon = 11;4$, the correction in (4) would be $-\,4;2{,}36{,}40^\tau$ or very close to $-\,4^\tau$ which is used in ephemerides.

The value (1) of $\Delta\lambda$ and (3*b*) of $\Delta\tau$ is used in an ephemeris[51] for Ξ which is thus seen to represent mean motion only. No. 401 operates even in steps of 8 years (or 5 phenomena) following (2) and (4). This is our only evidence concerning "System A_0."

32. Both System A_1 and System A_2 make use of the periodicity after 5 phenomena of the same kind ("8-year" period). In System A_2 the decrease in longitude after 5 lines is $2;30°$, as expected from (2), whereas System A_1 uses $2;40°$ instead—for reasons unknown to me. For the time difference both systems assume $-\,4^\tau$. But A_1 and A_2 not only differ in the final correction after 5 lines, but also somewhat in the distances between the intermediate 5 positions and dates, though the deviations are slight.

The procedure in both systems is quite simple. For each phenomenon and for each zodiacal sign (or group of consecutive signs), a synodic arc is prescribed such that the total of five consecutive arcs is $2;30°$ or $2;40°$ less than a complete rotation. Similarly for dates, resulting after 5 steps in a decrease by 4^τ. For each phenomenon, 5 coefficients for longitudes and 5 for dates are given, thus 60 coefficients for all six phenomena.

A complete list of these coefficients was given at the end of the ephemeris No. 421*aa* (System A_2), the present state of preservation of which is shown in figure 5 (p. 75). A complete control of both systems would thus require the knowledge of 120 coefficients. Existing ephemerides and the fragmentary list just quoted allow us to

[49] Pañca Siddhāntikā XVIII, 1. Dr. Olaf Schmidt informed me that the translation on p. 97 gives in error "the third part of a degree" for "one half of a degree."

[50] *Cf.* below, p. 80.

[51] No. 400.

reconstruct about one-third or one-fourth of these parameters. The best known part concerns the phenomena Ξ and ☿ in System A_1 and ☿ and Ω in System A_2 as given in table 3. The meaning of these tables is as follows. Suppose that for an ephemeris of System A_2, ☿ falls at ♉ 25;10. Then our list of coefficients tells us that the next ☿ will be 3,37;30° or 7 signs and 7;30° farther ahead, that is, in ♋ 2;40. For a position in ♋ we find as next ☿ from our table at point 7ª − 1;30 ahead, thus ♌ 1;10. Then follows ♌ 1;10 + 7ª + 8;30 = ♓ 9;40, then ♎ 23;10 and finally ♉ 22;30 i.e., a position 2;40° before the previous position in Taurus. If we had used the scheme for ☿ of System A_1, we would have ended up in ♉ 22;40 since the only difference between the two tables consists in the arc for ♎ which is 7ª − 0;40 in System A_2, 7ª − 0;30 in System A_1. A similar procedure holds for the dates, resulting in both systems in a day number that is 4ᵗ less after 5 steps.

In the preceding example we have used every line of the scheme exactly once. If this is the case, the total effect of 5 steps for every phenomenon is a decrease in longitude by 2;40° in System A_2, by 2;30° in System A_1, and by 4ᵗ in the dates for both. It is, however, easy to see that this scheme must not always lead to the desired changes after one 8-year period. If we had started, e.g., with ♈ 10, we would end up with ♈ 1;20 instead of ♈ 7;20. The existing ephemerides are too fragmentary to answer the question how one proceeded in cases where one line of the scheme would be used twice while another would by bypassed.

TABLE 3

	System A_1			
	Ξ		☿	
Δλ	♓	3,44,10° = 7ª + 14,10°	♉	3,37,30° = 7ª + 7,30°
	♌	3,30,30 = + 0,30	♋	3,38,30 = + 8,30
	♎	3,32 = + 2	♍♎	3,29,30 = − 0,30
	♉	3,36,20 = + 6,20	♐	3,28,30 = − 1,30
	♓	3,34,30 = + 4,30	♒	3,43,30 = + 13,30
Δτ	♓	10,0ᵗ = 20ᵐ + 0ᵗ	♉	10,1ᵗ = 20ᵐ + 1ᵗ
	♌	9,23 = − 37	♋	9,59 = − 1
	♎	9,45 = − 15	♍♎	9,46 = − 14
	♉	10,11 = + 11	♐	9,49 = − 11
	♓	10,7 = + 7	♒	9,51 = − 9

	System A_2			
	☿		Ω	
Δλ	♈ ♉ ♓	3,37,30° = 7ª + 7,30°	♈ ♉ ♓	3,36° = 7ª + 6°
	♋ ♌	3,38,30 = + 8,30	♋ ♌	3,38 = + 8
	♍♎ ♎	3,29,20 = − 0,40	♍♎ ♎	3,32 = + 2
	♐ ♉	3,28,30 = − 1,30	♐ ♉	3,29,30 = − 0,30
	♒ ♓	3,43,30 = + 13,30	♒ ♓	3,41,50 = + 11,50
Δτ	♈ ♉ ♓	10,1ᵗ = 20ᵐ + 1ᵗ	♈ ♉ ♓	9,58ᵗ = 20ᵐ − 2ᵗ
	♋ ♌	9,59 = − 1	♋ ♌	10,4 = + 4
	♍♎ ♎	9,46 = − 14	♍♎ ♎	10,1 = + 1
	♐ ♉	9,49 = − 11	♐ ♉	9,40 = − 20
	♒ ♓	9,51 = − 9	♒ ♓	9,43 = − 17

33. The 8-year interval for which corrections of − 2;30° or − 2;40° and − 4ᵗ are required can also be called an "approximate period" of Venus. It is a well known period also in Greek astrology, e.g., Vettius Valens III, 14 (p. 115,18 Kroll) or IV, 30 (p. 206,2). As theoretically exact, the period which can be derived from the mean synodic arc[52] Δλ = 3,35;30 by means of (2) p. 66 was accepted, namely,

$$(5a) \quad P = \frac{6,0}{3,35;30} = \frac{12,0}{7,11} = 1;40,13, \ldots$$

or

(5b) 720 occurrences = 1151 years.

This period of 1151 years also recurs in Greek astrology[53] and in the Pañca Siddhāntikā.[54]

Similar to the 8-year period, several other approximate periods for Venus were known and the corresponding corrections listed in procedure texts;[55] at least three values are preserved: 71 years, 95 years, and 261 years, but several more must have been quoted in partially destroyed passages.

34. It is also only from fragments of procedure texts that we know at least parts of the connections which were assumed between consecutive phenomena of different kind. For example, we are told that the increase in longitude from Γ to Σ is

5,27° in ♍	5,27° in ♉
5,28;30 in ♌	5,28 in ♒
5,30 in ♍♎	5,30 in ♓
5,31;30 in ♎	5,29 in ♓

probably starting from the sign of Γ. Similar rules existed for the dates; we know, e.g., that the duration of invisibility at superior conjunction (from Σ to Ξ) was assumed to be

in	♍ to ♏	56ᵗ
	in ♐	1,0
in	♉ and ♒	1,2
	in ♓	1,1
in	♈ and ♉	1,0
	in ♊	58

[52] From No. 400.
[53] CCAG 1: 163, 19.
[54] XVII, 76.
[55] Nos. 812 and 816.

At least 14 sections of a large procedure text[56] were devoted to the enumeration of such coefficients for longitudes and dates.

This method is by no means the only one to be followed in the transition from one phenomenon to the next one. The same procedure text continues with a rule which indicates that (for Ξ) in ♍ the arc from Ξ to ψ is 4,19;15 followed by a retrogradation of 6;15 from ψ to Ω. Thus an appearance as morning star in the beginning of ♍ would be followed by a station at the end of ♉ and a disappearance Ω about the middle of ♉. But Ω will not always be at a mean distance of 4,19;15 − 6;15 = 4,13;15 from Ξ. Our text gives corrections to be added or subtracted to the mean position of Ω. These corrections are

for Ω in ♊ : + 1°	for Ω in ♐ : − 2°
♋ : + 3	♑ : − 3;30
Ω : + 3;30	♒ : − 3;30
♍ : + 4	♓ : − 3
♎ : + 2	♈ : − 2
♏ : + 1	♉ : − 1

This shows that no correction is needed for an Ω at the end of ♉, in agreement with our initial statement for the relations between Ξ, ψ, and Ω for Ξ in ♍.

Similar corrections are given for the subsequent reappearance Γ, which I shall, however, not list here because there are too many open questions in this very difficult section. All in all, it is clear, however, that one can easily construct internal contradictions between all these various systems similar to the inconsistencies which we derived for Mercury by the simultaneous application of System A_1 and A_2.

The fragmentary character of all our texts for Venus makes it pointless to discuss these discrepancies numerically here. It cannot be doubted, however, that a great number of competing methods for ephemerides of Venus existed, the mutual relationship of which is far beyond our present grasp.

MARS

35. Our information for the theory of Mars is about as insufficient as in the case of Venus. This much, however, seems to be clear: a sharp distinction was made in the procedure for the parts of the orbit with direct motion and for the retrograde arc. This very definitely distinguishes the theory of Mars from the procedure for Jupiter and Saturn, where all phenomena are treated in the same fashion. The great irregularity in the appearances of the retrogradation of Mars is probably the reason for this distinction. The more remote a planet is from the sun and from the earth, the more its retrogradation approaches the character of a yearly parallax. No such simple consideration would work in the case of Mars.

The only well known system for the determination of longitudes and dates for appearance Γ, first station φ, and disappearance Ω is "System A" based on six zones of the ecliptic of two signs each, from Taurus and Gemini, . . . , to Pisces and Aries. This very same division is also known from Hindu astronomy.[57] We have, however, a small fragment of one ephemeris—just enough to show that it was not computed with System A. Hence we must be prepared to expect also for Mars a greater variety of methods than known so far.

The synodic arcs in System A, beginning with the sector Taurus-Gemini, are 45, 30, 40, 1,0, 1,30, and 1,7;30 respectively. From this it follows that one had assumed

$$(1a) \qquad P = \frac{2,13}{18} = 7;23,20$$

or

(1b) 284 years = 133 occurrences
= 151 rotations

and

$$(2) \qquad \overline{\Delta\lambda} = 48;43,18,30^\circ$$

for the mean synodic arc. But we also know[58] about the use of approximate periods of 47 and of 79 years.[59] The main period of 284 years is again known to Greek astrology.[60]

36. For the actually traveled distance between two consecutive occurrences of the same kind, one finds from (2)

$$(3) \qquad \Delta\Lambda = 6,0 + \overline{\Delta\lambda} = 6,48;43,18,30$$

and from this a synodic time

$$(4) \qquad \overline{\Delta\tau} = \overline{\Delta\lambda} + 12,23;37,52^\tau$$

by using the epact $\epsilon = 11;4,0,3, \ldots \approx 11;4^\tau$. From (3) and (4) one obtains as mean synodic

[56] No. 812.

[57] Pañca Siddhāntikā, ch. XVIII, 29–32.

[58] From No. 811.

[59] Kugler, SSB 1: 45 and Nos. 811 and 811*a*.

[60] CCAG 1: 163,18 (Rhetorius excerpting Antiochus).

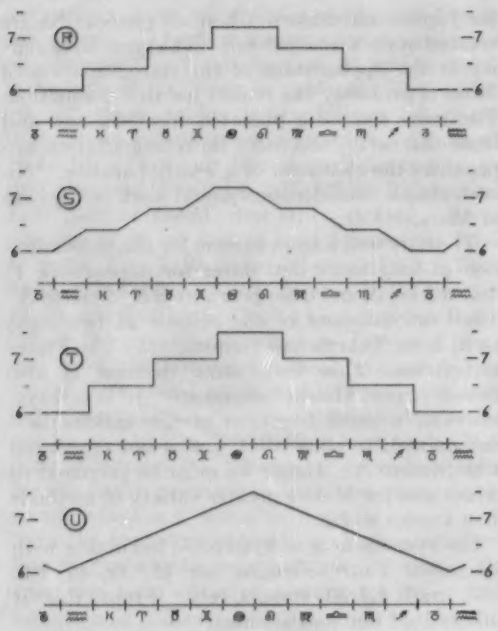

FIG. 8.

time

$$\overline{\Delta\tau} = 13{,}12;21{,}10{,}30^{\tau}.$$

From a procedure text[61] we know a still more accurate value, namely,

(5) $$\overline{\Delta\tau} = 13{,}12;21{,}10{,}22{,}47{,}20^{\tau}$$

which shows to what degree the mathematical consequences of the theory were developed.[62]

37. As stated before, System A is only applied to Γ, ϕ, and Ω. For the retrograde motion from the first station ϕ to opposition Θ and second station ψ, entirely different rules are used. The situation is quite analogous to the relation between primary columns and "pushes" in the theory of Mercury. We now may assume that longitudes and dates of ϕ, have been determined. Then, depending on the longitude of ϕ, the length of the retrograde arc from ϕ to Θ is prescribed by one of four schemes, graphically represented in figure 8. The methods R and T are based on the same zones as System A, with 6° as minimum arc from ϕ to Θ and with a maximum of 7;12 in R, of 7;30 in T. Method S is a partial linearization of R, method U a complete linearization of T.

We are much less well informed about the second half of the retrograde motion, from Θ to ψ. All that we know is that for method S the arc from Θ to ψ is assumed to be $\frac{3}{4}$ the arc from ϕ to Θ found previously. Similar rules for the total retrogradation of Mars are given in the Pañca Siddhāntikā,[63] in eaxct agreement with the results obtained from method R, if we use the ratio $1:\frac{3}{4}$ for the two segments before and after opposition.

Less is known about the corresponding dates. We have, of course, to consider the date of ϕ to be given. In No. 500 the dates of Θ are found by adding the constant amount of $47;55{,}4^{\tau}$ to the dates of ϕ. Nothing can be said about the time from Θ to ψ or about possible variants. The existence of at least four methods for the computation of the first half of the retrogradation makes it very plausible that a similar variety of methods existed for the second half and for the dates.[64] However incomplete our knowledge of these variations may remain, it suffices to demonstrate the futility of speaking about "the Babylonian planetary theory" in the same sense as one can speak about the Ptolemaic theory.

38. As described in the previous section, Θ and ψ are determined, at least in principle, as soon as ϕ is known. System A itself, however, gives no information as to the initial relation between Γ, ϕ, and Ω. Such information is supplied by rules found in procedure texts, again in at least two variants.

The first system of rules[65] gives $2{,}42;40°$ and $4{,}40^{\tau}$ as distance and time interval between Γ and ϕ, assuming that Γ falls either in ♈ or ♎. Similarly $3{,}11;20°$ and $5{,}23^{\tau}$ lead from ψ to Ω. For the period of invisibility we only know that the mean motion is $0;40°/^{\tau}$ with a maximum of $0;50°/^{\tau}$ in ♉ and a minimum of $0;30°/^{\tau}$ in ♋. Also a retrograde velocity of $-0;12°/^{\tau}$ is given, which obviously disagrees with the more detailed scheme quoted before.

The second system of rules[66] applies to mean motion only, but is based on very accurate the-

[61] No. 811*a*.

[62] The equivalent of (5) in days is about 779.955^{d} instead of 779.936^{d}.

[63] XVIII, 29–32.

[64] We even have two (related) tablets which give longitudes but no dates. Since it is possible that the two texts cover one complete period of 284 years we might have here a type of procedure text rather than an ephemeris without dates.

[65] From No. 811*a*.

[66] Again from No. 811*a*.

oretical considerations. The discussion is based on the parameter

$$c = 6,23;37,52^{\tau} \tag{6}$$

which must be added to the mean synodic travel $\overline{\Delta\Lambda} = 6,48;43,18,30$ (*cf.* (3), p. 81) in order to give the synodic time

$$\overline{\Delta\tau} = \overline{\Delta\Lambda} + c. \tag{7}$$

The value of c is now split up into three components, corresponding to the segments of the motion of Mars from Ω to Γ (invisibility), to ϕ (first station), and back to Ω:

$$\begin{array}{ll} \text{from } \Omega \text{ to } \Gamma: & c_1 = 33;40,4^{\tau} \\ \text{from } \Gamma \text{ to } \phi: & c_2 = 1,53;13,13 \\ \text{from } \phi \text{ to } \Omega: & c_3 = 3,56;44,35. \end{array} \tag{8}$$

The reason for exactly this division is unknown. But it is very characteristic for the theory of Mars that the retrograde motion is excluded from this procedure. Obviously there must have existed a similar subdivision of $\overline{\Delta\Lambda}$ such that for each sector

$$\Delta_i\tau = \Delta_i\Lambda + c_i$$

holds. After some damaged passages, difficult to understand, a subdivision of the whole synodic time is carried out. The mean synodic travel $\overline{\Delta\Lambda} = 6,48;43,18,30$ is multiplied by

$$\frac{\epsilon}{6,0} = \frac{11;4}{6,0} = 0;1,50,40 \tag{9}$$

with the result

$$\mu = 12;33,51,52,47,20^{\tau}. \tag{10}$$

This number is again divided into three components, first accurately, then approximately:

$$\begin{array}{ll} \mu_1 = 2;44,45,6,46[\ldots & \\ \qquad\qquad \approx 2;44,45^{\tau} & \\ \mu_2 = 4;59,22,55,47,46,40 & \\ \qquad\qquad \approx 4;59,33 & \\ \mu_3 = 4;49,33,5[0,\ldots,]51,6,40 & \\ \qquad\qquad \approx 4;49,33,50. & \end{array} \tag{11}$$

The readings of the exact numbers are not complete because of destroyed signs, and in μ_2 the 22 must be an error for 32.

The significance of the parameter

$$\mu = \frac{\epsilon}{6,0} \cdot \overline{\Delta\lambda} \tag{12}$$

can be derived easily from our general formulae. If we substitute in (13) p. 68 (with $i = 2$ for Mars)

$$\overline{\Delta\tau} = \left(2 + \frac{1}{P}\right)(6,0 + \epsilon) \tag{13}$$

the value

$$\frac{1}{P} = \frac{\overline{\Delta\lambda}}{6,0} \tag{14}$$

from (14*c*) p. 68, we then obtain

$$\overline{\Delta\tau} = \overline{\Delta\lambda} + 2(6,0 + \epsilon) + \frac{\epsilon}{6,0} \cdot \overline{\Delta\lambda} \tag{15}$$

This formula indicates that the mean synodic time is the sum of the mean synodic arc (counted as tithis) plus twice the length of one year (because $6,0 + \epsilon = 6,11;4^{\tau} = 12;22,8^{m}$) plus a correction which is, according to (12), the parameter μ.

The general relation

$$\overline{\Delta\tau} = \overline{\Delta\Lambda} + (6,0 + \epsilon) + \mu$$

is now divided into the three components

$$\begin{array}{l} \Delta_1\tau = \Delta_1\Lambda + c_1 \\ \Delta_2\tau = \Delta_2\Lambda + c_2 \\ \Delta_3\tau = \Delta_3\Lambda + c_3 \end{array} \tag{16}$$

and it is shown that the values (8) for the c_i satisfy the relations

$$c_i = r_i\left(1 + \frac{\epsilon}{6,0}\right) + \mu_i \tag{17}$$

where the values for μ_i are the abbreviated values in (11) and

$$\begin{array}{l} r_1 = 30 \quad r_2 = 1,45 \quad r_3 = 3,45 \\ r_1 + r_2 + r_3 = 6,0. \end{array} \tag{18}$$

Unfortunately the text is not well enough preserved to yield the values of $\Delta_i\Lambda$ and $\Delta_i\tau$. We could nevertheless compute them if we were entitled to assume that for each segment

$$\Delta_i\Lambda = \frac{6,0}{\epsilon}\mu_i$$

holds, as in (12) for the whole synodic arc.

The incompleteness of the text prevents us from penetrating into all the details of the theory, but it is at least clear that we are dealing here with a strict mathematical theory of the basic relations between synodic arc and synodic times and the subdivisions of the direct motion of the planet.

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39. From procedure texts and ephemerides for the five phenomena Γ, ϕ, Θ, ψ, Ω, we know of the existence of three systems of type A (plus some variants) and two of type B. We furthermore have one large ephemeris for day-to-day motion. Many rules found in procedure texts are not reflected in the preserved ephemerides. Future investigations will have to show whether some of these rules perhaps apply to other classes of texts, e.g., Almanacs.

The most important period is

$$(1a) \qquad P = \frac{6,31}{36} = 10;51,40$$

or

$$(1b) \quad 391 \text{ occurrences} = 427 \text{ years} = 36 \text{ rotations}$$

used in Systems A, A′, and B. It also occurs in Greek astrology[67] and in the Pañca Siddhāntikā.[68] The mean synodic arc which results from (1*a*) is

$$(1c) \qquad \frac{6,0}{P} = 33;8,44,48, \ldots$$

As approximate periods appear 12 years, 71 years, 83 years, 95 years, and 261 years[69] whereas a period of 344 years, found by Kugler,[70] is not yet attested in the texts discussed here. Variants of (1*a*) will be mentioned in the discussion of System B′ (*cf.* p. 85).

40. System A operates with two arcs only, with ♐ 0 and ♊ 25 points of discontinuity. The two synodic arcs are

$$(2) \qquad \begin{aligned} W &= 36° \text{ from } ♐\ 0° \text{ to } ♊\ 25 \\ w &= 30 \text{ from } ♊\ 25 \text{ to } ♍\ 30. \end{aligned}$$

I call these two arcs of the ecliptic "fast arc" and "slow arc" respectively. The period of (2) is given by (1*a*).

From a procedure text[71] we known of a modification of System A to the effect that the boundary between the two arcs depends on the phenomena in question. For the motion from Ω to Γ (invisibility), ♊ 27 and ♐ 5 are the points of discontinuity; after Γ, however, ♋ 0;45 and ♐ 5;45 are used. Unfortunately, not enough details are known for a complete reconstruction of this variant.

The computation of the dates in System A is based on the relation

$$(3a) \qquad \Delta\tau = \Delta\lambda + 6,12;5,10^{\tau} = \Delta\lambda + 12^{m} + 12;5,10^{\tau}.$$

Since $\Delta\lambda$ is either W or w according to (2), consecutive dates on the fast arc have a difference of

$$(3b) \qquad D = 12^{m} + 48;5,10^{\tau}$$

and on the slow arc

$$(3c) \qquad d = 12^{m} + 42;5,10^{\tau}.$$

As we shall see later, (3*a*) is only a rounding-off of a more accurate relation

$$(4) \qquad \Delta\tau = \Delta\lambda + 6,12;5,8,8,20^{\tau}$$

which occurs in a procedure text.[72]

System A′ deviates from System A by introducing an intermediate arc between fast and slow arc as follows

$$(5) \qquad \begin{aligned} W &= 36 \quad \text{from } ♉\ 9 \text{ to } ♉\ 17 \\ w' &= 33;45 \text{ from } ♉\ 17 \text{ to } ♋\ 9 \\ w &= 30 \quad \text{from } ♋\ 9 \text{ to } ♍\ 9 \\ w' &= 33;45 \text{ from } ♍\ 9 \text{ to } ♉\ 9 \end{aligned}$$

The period is again (1*a*). Also the dates follow the procedure of System A, now leading to three values for the time intervals

$$(6) \qquad \begin{aligned} D &= 12^{m} + 48;5,10^{\tau} \\ d' &= 12^{m} + 45;50,10 \\ d &= 12^{m} + 42;5,10 \end{aligned}$$

with d' being used on both intermediate arcs.

Different values, at least for d and d', are derived in a procedure text[73] in the following fashion. The synodic arc (30° and 33;45 respectively) is multiplied by $1;1,50^{\tau}$ which is the time needed for the mean sun to travel 1°. To this product, which represents the time for the sun to cover the synodic arc, is added the epact ϵ which gives the excess over 12^{m} in one complete rotation of the sun. Surprisingly, the value of ϵ is taken to be $11;3,20^{\tau}$ in the computation of d, as $11;3,45^{\tau}$ in the computation of d'. Thus it is found that

$$d = 41;58,20^{\tau} \quad d' = 45;50,12,30^{\tau}.$$

Since the text shows several evident errors,[74] the

[67] CCAG 1: 163,18.
[68] XVIII, 6.
[69] In Nos. 812, 813 and 814.
[70] SSB 1: 49.
[71] No. 813.
[72] No. 812.
[73] No. 813. The result for D is not preserved.
[74] The value of d appears, e.g., as 41;48,20. Of interest is the term gaba-ri mu-an-na which is applied to these synodic times.

values of ϵ cannot be taken too seriously, especially since $\epsilon = 11;4^{\tau}$ is explicitly used in similar computations for System B of Jupiter.

System A′, however, is not the only way of modifying the two-arc arrangement of System A by introducing intermediate arcs. From procedure text No. 811 we know of a System A″ with synodic arcs of 32° in ♊ and ♍ followed by 34° in ♐ and preceded by 34; . . . (?) in ♉. From ♋ to ♎ we have again 30° and from ♏ to ♈ we may restore 36°.[75]

Beside these three major systems, we know of at least two minor modifications of System A′ from consecutive sections of the same procedure text.[76] The synodic arcs have the same values as in (5) but in one system the intermediate arcs extend

$$(7)\qquad \begin{matrix}\text{from } ♉\ 17 \text{ to } ♋\ 5;45 & \text{and}\\ \text{from } ♐\ 5;45 \text{ to } ♏\ 2\end{matrix}$$

an arrangement which leaves the period (1*a*) unchanged. In the other system the only change in (5) consists in the replacement of ♏ 2 by ♐ 27, thus increasing the length of the fast arc at the expense of one intermediate arc. Consequently, the period obtains the value

$$(8)\qquad P = 10;51,6,40$$

which might be the purpose of this modification.

41. "System B" is based on another idea. No longer is a direct relation between the amount of the synodic arcs and the zodiac maintained, but the sequence of the synodic arcs and synodic times is prescribed to follow a linear zigzag function. This zigzag function is based on the following extrema

$$(9a)\qquad \begin{matrix}m = 28,15;30\\ M = 38,2\end{matrix}$$

with a difference

$$(9b)\qquad d = 1,48$$

resulting in the correct period

$$(9c)\quad P = \frac{2(M-m)}{d} = \frac{6,31}{36} = 10;51,40.$$

The mean value is

$$(9d)\qquad \mu = \tfrac{1}{2}(m+M) = 33;8,45$$

which is obviously an approximation of (1*c*).

As we have shown in (8) p. 70 the relation

$$6,0\cdot d = (M+m)(M-m)$$

should be satisfied. Substituting the values of (9), we find

$$\begin{matrix}6,0\cdot d = 10,48\\ (M+m)(M-m) = 10,48;0,3,45.\end{matrix}$$

This again shows that the parameters (9) deviate by a minute amount from the theoretically exact values.

In System A and its variants, the synodic times were related to the synodic arcs by

$$\Delta\tau = \Delta\lambda + 12^{m} + 12;5,10^{\tau}$$

(*cf.* (3*a*) p. 84) where 12;5,10 is a convenient approximation of 12;5,8,8,20 (*cf.* (4) p. 84). Since the mean synodic arc was defined as 33;8,45 we now should expect $45;13,55^{\tau}$ as mean value for the dates. In fact, however,

$$(10a)\qquad \mu = 45;14$$

was used. Since the differences between synodic arcs and synodic times must be the same, we have again

$$(10b)\qquad d = 1,48.$$

And because P also must be kept unchanged (9*c*), the values of m and M are computable from

$$\begin{matrix}m + M = 2\,\mu\\ m - M = \tfrac{1}{2}\,Pd.\end{matrix}$$

In this way one finds

$$(10c)\qquad \begin{matrix}m = 40;20,45^{\tau}\\ M = 50;7,15\end{matrix}$$

for the extremal values of the synodic times, of course adding 12^{m} in all cases.

Beside System B, which is often represented in ephemerides, we know from procedure texts[77] and from one ephemeris[78] the existence of a variant, System B′, with the following parameters

$$(11a)\qquad \begin{matrix}m = 28;19,10 & \mu = 33;8,45\\ M = 37;58,20 & d = 1;46,40.\end{matrix}$$

The mean value is the same as in System B (9*d*), but the change of the extrema changes the period to

$$(11b)\qquad P = \frac{11,35}{1,4} = 10;51,33,45.$$

[75] The resulting period is increased to 10;58, . . . from 10;51,40.

[76] No. 813.

[77] Nos. 813, 816, 820.

[78] No. 640.

One might think that this change was made in order to reach better agreement between mean synodic arc and period. Applying the same criterion as before, however (*cf.* p. 85), we find

$$6{,}0 \cdot d = 10{,}40$$
$$(M + m)(M - m) = 10{,}39{;}53{,}55{,}25$$

thus a difference of $-0;6,4,35$ instead of $+0;0,3,45$ as previously.

As to the dates in System B′, the mean synodic time is chosen as

$$(12a) \qquad 33;8,45 + 12;5,8 = 45;13,53^{\tau}$$

which can be considered an improvement over the procedure in System B ($10a$). The remaining parameters are

$$(12b) \qquad \begin{aligned} m &= 40;24,15 \\ M &= 50;3,31 \\ d &= 1;46,40 \end{aligned}$$

which lead to a period

$$(12c) \qquad P = \frac{2,24,49}{13,20} = 10;51,40,30.$$

Though in practice of little importance, it is theoretically objectionable that longitudes and dates do not have exactly the same period.

42. All methods discussed so far apply equally to all the five phenomena which are considered in ephemerides for Jupiter. Again the problem arises as to how the initial values for all these phenomena were determined. The answer seems to be given by those sections in procedure texts which enumerate the differences in longitude and in time between consecutive phenomena like Γ and ϕ, and Θ, etc. or the duration of invisibility as function of the zodiac. In fact, however, we have preserved so great a variety of rules of this type that no precise answer seems to be possible in view of the many combinations of systems and rules. And to this must be added the fact that the initial values which occur in the preserved samples disagree with the known rules more often than not. Thus the comparative abundance of ephemerides and procedure results in an *embarras de richesse* rather than in a clearer insight into the computational techniques which produced the ephemerides.

There exist, nevertheless, some cases where we see clearly that one had the intention of hinging a complete ephemeris for all phenomena on one single pair of data (longitude and date) for any one arbitrarily chosen phenomenon. This is shown by the fact that we have rules for the transition from one phenomenon to the next (say, from Γ_0 to ϕ_0 to Θ_0, etc.) such that the final return to the same type (Γ_1) exactly agrees with the result one obtains by going directly from the initial phenomenon to its next occurrence (from Γ_0 to Γ_1). For example, the following scheme can be abstracted from procedure texts for System A′

TABLE 4

Segment	Time	Motion in Longitude		
		slow	medium	fast
$\Omega \to \Gamma$	30^{τ}	$6;15°$	$7;1,52,30°$	$7;30$
$\Gamma \to \Gamma'$	30^{τ}	$6;15$	$7;1,52,30$	$7;30$
$\Gamma' \to \Phi$	3^{m}	10	$11;15$	12
$\Phi \to \Psi$	4^{m}	$-8;20$	$-9;22,30$	-10
$\Psi \to \Omega'$	3^{m}	$9;35$	$10;46,52,30$	$11;30$
$\Omega' \to \Omega$	30^{τ}	$6;15$	$7;1,52,30$	$7;30$
Total	13^{m}	$30°$	$33;45$	36

The points Ω′ and Γ′ have no special significance beyond the fact of their being endpoints of a segment where the planet moves with constant velocity[79] and which includes the interval of invisibility from Ω to Γ. These points are called "30 days before its (the planet's) setting" and "30 days after its rising" whereas the interval of invisibility is referred to as "with the sun." This proves that one had realized that the invisibility of a planet is caused by its proximity to the sun.

The data of the above-given table often appear in the procedure texts in the form that not travelled distances but time and velocities are given. We have already quoted the three-month interval from Ω′ to Γ′ where we have a constant velocity of $0;12,30^{\circ/\tau}$ under the assumption that both endpoints belong to the slow arc. Since the synodic arcs in System A′ have the ratio of

$$30:33;45:36 = 40:45:48$$

one uses the same ratios for all segments of the intermediate motion. Thus the planet is supposed to move from Ω′ to Γ′ with the velocity $0;14,3,45^{\circ/\tau}$ on the medium arc, and with $0;15^{\circ/\tau}$ on the fast arc and similarly on all other segments, including, of course, retrogradation.

The total motions agree exactly with the

[79] Similar subdivisions are also found in the Pañca Siddhāntikā XVIII, not only for Jupiter but also for the other planets.

synodic arcs of System A′. The synodic time is in all cases expressed in round figures with a total of 13 months, which is the rough equivalent of a 12-year period of Jupiter. In many cases the scheme is left incomplete for the stretch "with the sun," giving the daily velocity only. This probably means that the time interval from Ω_0 to Ω_1 is known anyhow by direct computation of column Ω. The purpose of the time intervals listed for the subsequent segments is to obtain a proper estimate for the velocities there.

A great variety of such rules is known and in various degrees of accuracy. For the slow arc in System A, e.g., we have[80]

from Γ to ϕ:	+ 16;15°
from ϕ to Θ:	− 4
from Θ to ψ:	− 6
from ψ to Ω:	+ 17;45
from Ω to Γ:	+ 6

with the proper total of 30°. In other cases emphasis is laid on the time intervals; for the fast arc, e.g., we find[81]

from Ω to Γ:	29^{τ}
from Γ to ϕ:	$4^{m}4^{\tau}$
from ϕ to Θ:	58^{τ}
from Θ to ψ:	$2^{m}4^{\tau}$
from ψ to Ω:	$4^{m}10^{\tau}$

with a total of $13^{m}15^{\tau} = 6{,}45^{\tau}$ which is explicable as the mean value

$$\overline{\Delta\tau} \approx 12^{m} + 33 + 12^{\tau}$$

where 33 is a rounding-off for $\overline{\Delta\lambda} = 33;8{,}45$ and $12 \approx 12;5{,}10^{\tau}$.

It is quite obvious that the accuracy of these schemes is inferior to that of the ephemerides. At the same time, it is clear that they must furnish the basis for the proper arrangement of the initial values from which an ephemeris has to start, and it is plausible to assume the existence of rules which provided the same accuracy for the horizontal arrangement of values in an ephemeris as was required for the vertical columns.

43. The final step in the direction of passing from one phenomenon to the next lay in the computation of the day-by-day positions of Jupiter. A large tablet which is devoted to this problem shows the arithmetical skill with which a smooth change of velocity near the stationary points ϕ and ψ was obtained. A part of this fragment was known to Kugler and partially published;[82] two additional pieces were joined and copied by Pinches and are now finally reunited with the other fragment.[83] The text in its present state concerns the period from S.E. 147 IX to 148 V.[84]

Four consecutive columns always form a unit. The first gives the day numbers month by month—the days referring to the real calendar with months containing either 29 or 30 days,[85] not tithis as in the ordinary ephemerides. The fourth column contains the longitudes, the third their differences, the second the second differences which form an arithmetical progression of the first order.

44. In several procedure texts[86] for Jupiter there occur lists of coefficients, depending on the zodiacal signs, whose significance is not clear to me. A fully preserved section is headed by the sentence "According to a second (method), Jupiter moves forward 6,10 in Cancer (and then) disappears." The preceding section is badly damaged and ends in otherwise unknown parameters, but experience shows that the remark about a "second method" does not of necessity mean that the first method was explained in the preceding section since the procedure texts are often a quite unsystematic collection of different rules.[87] The obvious interpretation of the title would be that the distance from a second station (ψ) in ♋ to disappearance (Ω) is 6;10°. In fact, however, the distance from ψ to Ω is about three times as great.[88] The text then proceeds to give similar numbers for each zodiacal sign. These numbers form a linear zigzag function with a minimum $m = 6;10$ in ♎, maximum $M = 6;20$ in ♉, and mean value $\mu = 6;15$ in ♈ and ♎, the difference being $d = 0;1{,}40$.

The same list is concluded in No. 817 by an equally cryptic "for opposition, times 20; for

[80] No. 813. Of this scheme several variants exist, some affected by obvious errors. Also the example given here is perhaps not fully correct.

[81] Also in No. 813.

[82] SSB 2: 528f. though with some errors in readings and restorations.

[83] No. 654; probably also No. 655 belongs to the same tablet.

[84] A few minute fragments of similar texts were found in Uruk (ACT Nos. 650 to 653) but they all seem to be based on constant differences for the longitudes.

[85] We observed the same in the case of the daily motion of Mercury, above, p. 62.

[86] Main text No. 813 with close parallel in No. 817. Fragments of similar lists: No. 814 and No. 816.

[87] We even find in two texts for Jupiter problems concerning a trapezoid.

[88] *Cf.*, e.g., the table on this page.

second station, times 30'' which must mean that the above-mentioned coefficients should be multiplied by $\frac{1}{3}$ if concerned with Θ, by $\frac{1}{4}$ if concerned with Ψ. Again I do not understand the meaning of these rules.

SATURN

45. We have already described[89] the two-zone System A for Saturn which, by the way, is known to us so far from procedure texts but not from ephemerides. Its period is

$$(1) \qquad P = \frac{4,16}{9} = 28;26,40.$$

It is also known from Hindu astronomy.[90]

46. Exactly the same period is used for System B, the parameters being

$$(2) \qquad \begin{matrix} m = 11;14,2,30^\circ & \mu = 12;39,22,30^\circ \\ M = 14;4,42,30 & d = 0;12 \end{matrix}$$

which satisfy accurately the relation

$$6,0 \cdot d = (M + m)(M - m)$$

as required by theory.[91]

The synodic times form, of course, a linear zigzag function of the same period with a mean value

$$(3) \qquad \overline{\Delta\tau} = \overline{\Delta\lambda} + 11;27,20,37,30^\tau + 12^m.$$

This relation is based on the exact value 11;4 for the epact. From this, as in the case of Jupiter, the remaining parameters for the true synodic times can be derived, resulting in

$$(4) \qquad \begin{matrix} m = 22;41,23,7,30^\tau & \mu = 24;6,43,7,30^\tau \\ M = 25;32,3,7,30 & d = 0;12. \end{matrix}$$

The ephemerides are usually normed in such a way that both synodic arcs and times (and consequently also the longitudes and dates) show four sexagesimal digits only. This is made possible by starting from initial values which do not contain more than four places.

We do, however, have ephemerides with three places for the synodic times and dates[92] whereas the longitudes follow the above-given scheme with four places. The parameters for the synodic times are rounded-off from (4) to

$$(5) \qquad \begin{matrix} m = 22;41,25^\tau & \mu = 24;6,45^\tau \\ M = 25;32,5 & d = 0;12 \end{matrix}$$

Since the values of $M - m$ and of d are the same for (4) and (5), the period of (5) is again given by (1).

The parallelism between (5) and (4) is a good example of the caution which is needed in the interpretation of our source material. If we had, by accident, only (5) and not (4) at our disposal, we would combine (5) with (2) and deduce in this way a relation (3) with 11;27,22,30 instead of 11;27,20,37,30. This, in turn, would lead to a value of ϵ different from 11;4 and thus to some length of the year which is not 12;22,8^m. In fact, however, all these conclusions would not be valid because they are based on parameters slightly modified for the sake of shorter numbers. This is no empty warning; conclusions of exactly this type have repeatedly been used in order to demonstrate the existence of different types of years: sidereal, tropical, anomalistic. In fact, all that was actually demonstrated was the lack of insight into the mechanism of Babylonian astronomy.

47. As with the other planets, we meet with Saturn also the problem of initial values. Nothing is known for System B. For System A, however, two procedure texts[93] give a subdivision of the synodic motion into segments such that the ratio of the partial velocities is the same as for the synodic arcs:[94]

$$(6) \qquad w_1 : w_2 = 14;3,45 : 11;43,7,30 = 6:5.$$

The corresponding scheme follows:

	With the sun (Ω to Γ):	$0;6^{\circ}/\tau$	or	$0;5^{\circ}/\tau$ respectively
	30^τ after Γ:	0;6	or	0;5
	3^m to ϕ:	0;4	or	0;3,20
(7)	$52;30^\tau$ from ϕ to Θ:	$-0;5,4,24$	or	(?)
	60^τ from Θ to Ψ:	$-0;4$	or	$-0;3,20$
	3^m after Ψ:	0;4,18,40	or	0;3,35,30
	30^τ before Ω:	0;6	or	0;5

[89] *Cf.* p. 70.
[90] Pañca Siddhāntikā XVIII, 15.
[91] *Cf.* p. 69 f.
[92] Corresponding procedure texts are Nos. 801 and 802.
[93] Nos. 801 and 802.
[94] *Cf.* p. 70.

No time is assigned to the interval of invisibility (from Ω to Γ). In the line next to the last, the ratio (6) would require 0;4,18,36 instead of 0;4,18,40.

It is clear that a complete scheme of this type would allow us to start, e.g., with one Ω and to compute from it all subsequent phenomena Γ, ϕ, Θ, ψ. This then provides us with the initial values for all columns of an ephemeris, the subsequent lines of which can now be computed for each individual column. Obviously we need not have begun with Ω. Any one phenomenon, combined with (7), would provide us with a complete set of initial values. This is in fact the realization of the ideal of mathematical astronomy: from one initial situation, all future and all past events are determined.

I think one is fully justified in considering this strictly deterministic procedure as the real goal of the Babylonian astronomers. Only in practice the complexity of the arithmetical relations between columns and lines made certain approximations necessary. And the fragmentary character of the few texts at our disposal makes it quite impossible to separate sharply the theoretical requirements and the effects of all kinds of compromises in actually computed ephemerides.

In looking back, we can say that the theory of Saturn gives the impression of great consistency and completeness. It is only from the much larger material for the remaining planets that we learn to obtain a more adequate picture of the manifold of existing methods and the size of our ignorance.

Added in proofs (Jan. 1954). Prof. A. Sachs, who is at present in London, discovered two texts of great interest for this study: (*a*) he found the second half of the ephemeris and procedure text for Venus No. 421aa discussed above p. 79 f. This will give us the majority of the remaining parameters of System A_2 after baking and cleaning of the tablet; (*b*) he identified a small procedure text concerning Mercury. It turns out to represent a new, third, system of type A for this planet, operating with three zones and a period

$$P = \frac{18,39}{5,55} = 3;9,7,36, \ldots$$

which has to be added to the list (6) p. 66.

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